

University of Alberta

Organizational investigations:

An exploration of Wittgenstein's potential contribution to the study of organization.

by

Michael K. Mauws



A thesis submitted to the Faculty of Graduate Studies and Research in partial fulfillment
of the requirements for the degree of Doctor of Philosophy

in

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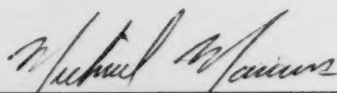
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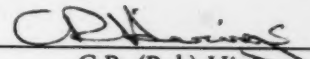
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mauws@compuserve.com

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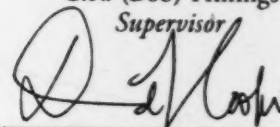
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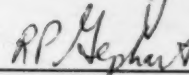
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C.R. (Bob) Hinings

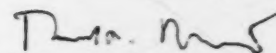
Supervisor



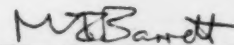
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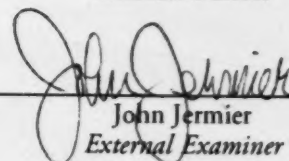
Robert P. Gephart, Jr.



Raymond Morrow



Michael Barrett


John Jermier
External Examiner

January 31, 1997

Before the work, the writer does not yet exist; after the work, he is no longer there.

- Maurice Blanchot

ABSTRACT

The body of this dissertation contains three papers. Although each of the three papers is intended to stand alone, what unites the three is their mutual origin in the ideas of Ludwig von Wittgenstein. And in this sense, the dissertation as a whole can be seen as an exploration of the potential implications of Wittgenstein's philosophy for the study of organization.

The first of these papers was written with Nelson Phillips. In this paper we intervene in a dialogue regarding Wittgenstein's concept of 'language games'. We provide what we feel is a richer understanding of this concept than was being utilized therein and, in doing so, address some of the criticisms being directed toward it. In addition, we suggest how this concept might further benefit those who research organizational phenomena.

In the second paper I begin by drawing attention to the origins and limitations of the 'information processing' metaphor that currently informs our understanding of organizational behavior. I then suggest that we may wish to consider an alternative conception of the 'subject', one informed by Wittgenstein's perspective on language. In this regard, I propose that we consider Pierre Bourdieu's concept of 'habitus'.

In the final paper I outline an alternative paradigm for organization studies. I argue that an alternative such as this is necessary because our existing "duality" paradigm creates false antinomies that impede research and obstruct collaboration. In response, I propose the "relationality" paradigm which transcends these antinomies and opens up new possibilities for organizational research.

The dissertation concludes with an examination of the moral implications of Wittgenstein's philosophy and connects these with recent writings on postmodernism and postmodern ethics. Because Wittgenstein's philosophy is so often associated with the 'demise of the ethical', I argue that it is imperative that we not also associate it with the 'demise of morality'.

To my parents, for allowing me to question.

And to Jim Pehura, for showing me how.

PREFACE

As I remember it, it was in the fall of 1991 that I first wandered into the office of Bob Hinings to discuss my dissertation topic. Some months earlier I had been greatly excited by the ideas contained in E. F. Schumacher's *Small is Beautiful*. For this reason, it was not particularly surprising that the topic I was interested in pursuing at that time was an exploration of optimal firm size. Simply stated, I was convinced that a great many firms reached a size that resulted in their experiencing dis-economies of scale. I was certain that these firms could be both more profitable and better corporate citizens if only they scaled down their operations. For this reason, I wanted to explore how it is that we might go about assessing what the optimal firm size for a given corporation might be.

Had I stuck with this topic, I suspect my dissertation would have been "hot stuff". For around that time, the business press was talking a great deal about "downsizing". Suddenly, or so it seemed, everyone was aware of the fact that most of our managerial knowledge revolved around helping companies grow. What they had realized is that very little was known about how a business might go about *reducing* the scope of its operations. Despite this situation having been acknowledged as much as ten years earlier, academics and practitioners alike had failed to explore the means whereby such strategies might be

realized. So as the effects of the economic recession that was upon us became ever more apparent, the call went out for guidance in navigating this new managerial terrain.

But as with all business fads – which ‘downsizing’ has now shown itself to be – practitioners were overly zealous in their embracement of this strategy. Providing an excellent example of the sort of insights we now associate with institutional theory, firms continued to downsize seemingly without regard for how it would affect the company’s overall well-being. For this reason, it was only a matter of time before the wake-up call was sounded in the form of “right-sizing”. And in light of this, as I put the finishing touches on this dissertation mid-way through 1996, I can only smile at how fortuitous my timing *might* have been had I stuck with that initial topic. I would, as the saying goes, have been “leading edge”.

But for better or for worse I chose to take that additional step forward that leads from the leading edge to the “bleeding edge”. And for this, with tongue in cheek, I can only blame my dissertation advisor, Bob Hinings. Why? Because on that fateful day in 1991 it was Bob who stepped up to the whiteboard and proceeded to try and translate what could only have been incoherent ramblings on my part into a series of boxes and arrows that might at least appear to make some sense. Given the veteran of theses and dissertations that he is, I have little doubt that Bob was practicing a tried and true method. And for this reason, I would not dare to question his intentions in adopting this approach. Nevertheless, it took only a few minutes of this before I had to ask that we bring an end to it and, in fact, that we bring an end to our entire discussion. Not because we were in any sense finished, mind you, but because it had become painfully clear to me was that there were a few things I needed to think about before I embarked upon my dissertation research.

Having never discussed this meeting with Bob, I can only assume that he saw this as pretty much normal behavior: new student brings in half-baked idea; student leaves with more questions than answers; a couple of weeks later student returns with a few more answers and a few less questions. But that said, in my case it appears that something may have gone a bit awry. For I don't think Bob and I ever did speak about 'optimal firm size' again. In fact, I really don't think there was ever a point after that meeting where I gave it any serious thought. Instead, from that day forward what would consume almost all of my attention was simply the question of how it might be possible that we could even *peak* of such a thing as a 'firm' or an 'organization'.

What needs to be mentioned here is that, as we sat down for that meeting, Bob had no way of knowing that Nelson Phillips and I were at that time struggling through Michel Foucault's *Archaeology of Knowledge*. Nelson and I had heard a great deal about Foucault up to that point, but neither of us really knew anything about him. So it was that we innocently asked Derek Sayer, who was teaching us a course in social theory in the fall of 1991, where we might commence our exploration of Foucault's ideas. And without so much as a scratch of the head, we were told that *Archaeology of Knowledge* was the place we wanted to start. So with this recommendation, we ordered our copies and anxiously awaited their arrival.

Looking back, I can only assume that Derek Sayer was being facetious in directing us toward this book.¹ For our own experiences aside, in a recent volume entitled *Foucault for Beginners* the only page devoted to this text contains an explicit warning that one should *not* commence one's investigations of Foucault's ideas with *Archaeology of Knowledge*. Unfortunately, such sound advice was not available to us in 1991 and, as a result, we

¹ At one point I informed Professor Sayer that Nelson and I had finished reading *Archaeology of Knowledge* and that we were, to say the least, a little confused. Although I do not recall what he actually said, I do remember a wide-eyed chuckle of disbelief when he realized that we had, in fact, taken him literally.

naively dove into our copies of *Archaeology of Knowledge* just as soon as we could get our hands on them.

In retrospect, it could only have been arrogance that got us through that book. For had we not been convinced that we were far too clever not to understand whatever might be put before us, we most certainly have set the book aside and gone on to other subjects or, at the very least, other sources. Nevertheless, we continued to get together after each and every chapter to determine if either of us had been able to make heads or tails of what was in that book. And although we had no way of knowing whether we were coming anywhere near to getting it "right", slowly but surely we did manage to make some sense of it.

In my own case, I might go so far as to say I made a little too much sense of the book. To wit, somehow or other I came to the conclusion that the entire book was really a methodology for the study of organization. Perhaps this was a reflection of the monomaniacal focus I had as a student of organizational analysis, or perhaps it was simply me trying to salvage the time I had invested in trying to understand it but, regardless, I was convinced that this book had a great deal to contribute to our discipline. In fact, the first conference paper I presented was little more than an attempt to map Foucault's analytical categories onto the phenomena of organization studies.

But as the one or two other individuals in the world who have read (and understood?) Foucault's book can testify, seeing it as a methodology for the study of 'organization' requires a very different idea of what we mean by 'organization'. In essence, it requires that one equate 'organization' with a 'discursive formation'— that we see 'organization' as being more or less an outcome of discourse. And although such perspectives are now beginning to take hold within our discipline, at that time there were few sources that someone thinking this way could consult. So with nowhere to look and no one to turn to,

for the most part I was walking around extremely confused about what these 'organizations' were that everyone, including myself, was talking about. Although we spoke about them as though they were some clearly bounded *thing* "out there" that we were discovering and getting to know, the image that was going through my mind had more in common with sediment settling on the bottom of the ocean. And out of this image, I was wrestling with the idea that what we were calling 'organization' might be a non-contiguous collection of particles from among this accumulating sediment. So with all this going on in my head, it is not surprising that I would experience more than my share of cognitive dissonance in Bob Hinings' office that fateful day in 1991.

What I found was that I started to squirm a wee bit more with each box Bob drew on the whiteboard. Each time he did so I found my mind wandering, thinking how absurd it was to represent something as complex as 'organization' with a mere rectangle on the board. As I sat there it seemed so obvious to me that, although this was our usual way of talking about and representing 'organizations', it had little if anything to do with the collection of practices from which this term had been abstracted. Thus, the inescapable conclusion I was arriving at was that, before I could talk about optimal firm size, I was first going to have to figure out what, exactly, I was going to be referring to each time I used the term 'organization'. And not surprisingly, as I excused myself from Bob's office, I had already made up my mind that the answer to this question was going to be found in this cryptic text I had been reading by Michel Foucault.

Even if had I never looked at Foucault's *Archaeology of Knowledge* again, just in light of this outcome it could be said that it had a significant effect on my thinking. But the fact remains that I did look at it again. And again and again and again. And each time I would find new insights in it, and each time I would become further impressed by the depth of understanding that it reflected. For in my attempts to understand this text I was exploring an ever-widening circle of social theory and philosophy, most notably the

emerging body of literature that is often identified as "poststructuralism". And in this sense, it is as much through the literature it led me to as through the ideas contained within it that *Archaeology of Knowledge* would come to define my entire way of thinking about social phenomena. It is in this way that I became familiar with writers such as Lyotard, Derrida, Lacan and Rorty. And most importantly, it is in this way that I would become familiar with the writings of Pierre Bourdieu.

My introduction to the work of Pierre Bourdieu came in the summer of 1992. How this would come about begins with my first class with Derek Sayer in early 1991. While taking it I had come to know another student in the class, Gürcan Koçan, from the department of political science. And what I would slowly discover was that, perhaps as a result of his difficulties with spoken English, Gürcan had gotten to know the library and, hence, social theory better than almost anyone I knew, including many of my professors. As a result, I often turned to him to gain ideas about other sources I might examine in my continuing attempts to understand the ideas expressed within *Archaeology of Knowledge*. So in this case, it was early in the summer of 1992 during one of my regular discussions with Gürcan that he told me that I might want to read Pierre Bourdieu's *Outline of a Theory of Practice*, for an increasing number of people seemed to be getting interested in his ideas. And so, with some free time on my hands, and the recommendation of someone whose opinion I very much respected, I took the book out of the library and proceeded to read it.

Initially, as I read the first few pages of *Outline* I had a strong feeling of *déjà vu* relating back to *Archaeology of Knowledge*. Not because the ideas were similar, mind you, but because once again I felt like I was reading a foreign language, despite its having been translated into English. The ideas were new, the prose was difficult and, as well, these were beautiful summer days that I was spending indoors. Even so, I persevered and made my way to the end of the book. And then proceeded to do so two more times.

Having read this book several times, and having read a great deal more of his work, I cannot begin to express what I have all learned and absorbed from my reading of Bourdieu. In fact, I have read so much of his work since then that my citations in no way convey his influence on my ideas. For as he himself says, his concepts are intended as heuristics rather than as suggestions for what might actually be "out there". They represent ways of thinking. And in my case, they are ways of thinking that now pervade most everything I write.

One of the most important effects that reading Bourdieu would have upon me was in awakening me to the significance of one's conception of the agent of social action. For it is through reading his work that I came to reflect back on some of the other writings of Foucault that I had been exposed to and, as well, upon Derek Sayer's *Capitalism and Modernity*, a penultimate draft of which I had read early in 1991. At that time, I thought I had understood the thesis that Derek was putting forth with his book. However, in retrospect I can now say that it was only after reading Bourdieu that I came to appreciate what his thesis actually was and, importantly, its significance. In brief, it was only then that I realized the importance of what he called the 'abstract individual', a phenomenon which he was arguing was made possible through the social practices associated with capitalism's rise. It was only then that I realized just what it was that had been made possible through the emergence of this 'abstract individual' and the central role it would have to play in any attempt to understand contemporary social outcomes such as complex organizations.

Discovering the 'subject' of social theory was, then, an important event in my intellectual development, and one for which I am very much indebted to the writings of Pierre Bourdieu (and Derek Sayer). Nevertheless, it is not this that I consider Bourdieu's most important contribution to my thinking. Instead, as a social scientist the most important insight that I have garnered from Bourdieu's writings is that my investigations

must not only take into account the social positions of those in whom I am interested but, as well, my own position within the field of intellectual production. For what he has made clear to me is that the topics on which I write are being dictated, to a large degree, by the particular field of relations in which I am positioned which, in this case, can be loosely identified as the field of organizational analysis. That this is so is particularly clear in our discipline in light of the fact that many of the topics on which we write have, quite often, been discussed years earlier within other fields of inquiry. Thus, it is only by virtue of our perceiving ourselves and each other as participating within a somewhat independent field of relations that addressing such topics can produce the outcomes that they do.

What this implies is that, as social scientists, an important part of our investigations involves articulating the position from which we speak. And in my own case, this has been extremely helpful. For as I eased myself into the turbulent epistemological waters of the 1990s, ever-weary of the seemingly nihilistic undertow of postmodernism, I found myself immersed in a sea of language. Everyone, it seemed, was worked up about our use of language. So worked up, in fact, that in the form of "political correctness" it had actually had an impact outside the "ivory tower". Thus, in an environment such as this, it was very easy for one to get washed away by the increasingly strong discursive currents. And admittedly, for a time I was caught up in these currents. I thought that 'discourse analysis' was the panacea when it came to understanding social reality and, in this sense, I was more or less convinced that there was nothing for us to explore other than language. In fact, I can distinctly recall Bob Hinings imploring me to acknowledge that there was more to organization than simply "talk".

Nevertheless, wading as deeply as I did into these waters has been extremely beneficial. For it allowed me to get beyond all the hype and, to some degree, to get beneath the surface to see what it was that was creating the ripples and waves that were visible on the surface. And in this regard, the source that I was led to was Wittgenstein's *Philosophical*

Investigations. For more than anyone else, I think it is Wittgenstein that can be credited for this shift taking place within the social sciences. However, Wittgenstein's work is anything but inviting and, as a result, it is through those that have been kind enough to "translate" Wittgenstein's work that it is becoming accessible and, thus, influential.

The accessibility is, however, misleading. For it is almost too easy to see Wittgenstein's later work as simply a philosophy of language, or a theory of meaning. And admittedly, this is something that I was guilty of for quite some time. For although Nelson Phillips and I chastise Graham Astley and Ray Zammuto in Chapter One for not going far enough in considering the ramifications of Wittgenstein's position, I think I might now say the same of our interpretations. I say this because, in discussing the implications of Wittgenstein's ideas for speech practices, we failed to appreciate and draw attention to their implications for *all* social practices and, in particular, their implications with respect to our knowledge of our 'selves'.

So it was by following Bourdieu's advice and trying to make my position within the intellectual field of relations visible to myself that I came to appreciate just how *radically* different Wittgenstein's ideas were. That is, by acknowledging that my own personal scale may have tipped just a little too far in favor of 'language', I came to see that language is, in the end, merely a tool we use to make our way in the world. Or to put it more starkly, I came to see that, although we can have a world without language, we cannot have language without a world.

But having said all this, I would be contradicting myself if I now thought that I had somehow stepped "outside" my own limited perspective. For this reason, let me make clear that the point of this self-reflexive exercise is not to seek any absolute answers but, rather, to draw attention to one's weaknesses and biases. And in this regard, the ideas expressed herein have been through one such round of self-examination. But the next, as

well as the ones after that, must be left to my readers, perhaps most notably myself at some point in the future. That said, I would hope that by making the effort to reveal something of the position from which I speak my readers will be better able to weigh and evaluate the potential contribution of all that is contained in the chapters that comprise this dissertation. Having shared with them those sources that I consider to have been most influential, I would hope that, although they may not agree with what I have written, they will at least have some understanding of how it is that I came to write it.

So toward this very same end, it is at this point that I would like to acknowledge the contributions to my thinking that cannot be conveyed through references and concepts. I would, for instance, like to note just how fortunate I am to have completed my studies at the University of Alberta, to have had access to what must undoubtedly be one of the finest libraries in Canada, to have been able to pursue my interests without the financial stress that so many other graduate students are forced to endure. In other words, I would like to say that I have benefited greatly by being at an institution whose commitment to academic excellence was, at least while I was there, unquestionable.

In general terms, I must also acknowledge the incredibly positive intellectual environment of the Faculty of Business and, in particular, the Department of Organizational Analysis. For although I more or less took it for granted while I was there, I have since come to realize just how fortunate I was to be amidst a group of faculty members who did not identify me by my status as a student but, rather, by the quality of my ideas. I have discovered, in brief, just how rare it is for graduate students to be treated in this manner. And as a result, I have also discovered just how much this can contribute towards one's intellectual development.

At a more personal level, I have been extremely fortunate in having been instructed by quite a number of what I would call "passionate intellectuals". By this I mean that I have

had the luxury of having associated with and taken classes from people who were truly excited about the material they were leading me through, people who in my eyes were in the classroom because they wanted to be rather than because this was what was necessary to make ends meet. These were people who were, quite simply, excited about ideas.

So at this point I would like to acknowledge some of these people, and the contribution they have made to both my thinking and my dissertation. In this regard, I have already acknowledged the contribution of Derek Sayer, through his book, his classes, and his advice(?) with respect to reading Foucault. I would at this time also like to acknowledge the guidance, support and friendship that resulted from the time I spent with Royston Greenwood. Although I never had any classes with Royston, he has, nevertheless, been an invaluable colleague over the past six years. As has been Lloyd Steier, who always showed a sincere interest in both me and my research, and who never let me leave his office without somehow providing me with a few words of encouragement.

There are four other faculty members I would like to thank, and I have had the good fortune of having all four of them on my dissertation committee. The first of these is Ray Morrow from the department of sociology who, along with Derek Sayer, helped me to realize that it is not the course title that matters but, rather, the name of the instructor. So as with Derek, I made sure to take every course Ray offered during my time at the U of A, regardless of whether I could get credit for it. And by doing so, I came to know the general contours of that vast terrain known as social theory.

To some degree, David Cooper also played a part in opening up this vista to me. Or perhaps more accurately, I might say that it was David that often provided me with my first opportunity to test out whatever new ideas I happened to be exploring. Simply stated, when it came to discussing new ideas, or exploring the ideas of important thinkers, David's door was invariably open. And as I have come to learn, to simply read about ideas

is seldom enough. In addition, what is often required is that we talk about them and, in this regard, David was an invaluable resource.

With respect to Bob Gephart, it is quite amazing the influence he has had upon my thinking when one considers that I never really took a class from him. Nevertheless, several of my peers had the good fortune of taking a class with him the year prior to my arrival in the Ph.D. program and, through his effect on them, Bob managed to have considerable influence on my own reading and thinking even before I had a chance to get to know him. But since that time Bob has, both through his own writings and through our conversations, continued to stimulate my thinking and to direct me toward new and fruitful streams of research. In addition, he has also done me the great service of making others aware of my work whenever the opportunity arises.

The final faculty member I would like to acknowledge is my dissertation advisor, Bob Hinings, who has contributed far more to my intellectual development and this dissertation than can be noted here. This is partially the result of his having contributed so much but, in addition, it is also the result of the manner in which he has contributed. For in large part, Bob's contribution has been in the form of process rather than content. With respect to the latter, he rarely intervened; this was, it seems, completely up to me. Nevertheless, he would sit me down periodically and remind me that my approach was not a conventional one and, moreover, that it was likely to adversely affect my employment prospects. But even so, as soon as I confirmed that exploring what I was interested in was far more important to me than securing a job, he would immediately tell me how pleased he was that I saw it this way. And so, through the respect he has shown for my ideas and the implicit encouragement this has provided me with – to say nothing of the explicit encouragement he has provided – Bob has had a tremendous influence in determining the sort of academic I am striving to become. And for this, more than anything else, I owe him a tremendous debt.

The other debt I owe is to those who were also students at U of A while I was there, as well as those who worked there. With respect to the latter, the guidance and assistance of Jeanette Gosine must be noted. My time as a Ph.D. student was unmarred by logistical nightmares and, for this, the credit must go to Jeanette. With respect to my classmates, some of them helped me out simply by joining me for a beer when things got too tough, others provided me with friendship when I had things to talk about, and some provided worthy opponents when it came time for academic debate. So to those like Gürcan Koçan, Shaun McQuitty and Gary Smith, I am deeply indebted. But an even bigger debt is owed to Thomas Lawrence and Nelson Phillips, who will undoubtedly prove to be lifetime friends and colleagues. Had I not met them, I suspect that this dissertation might never have been completed. At the very least, I can certainly say that it would not be *this* dissertation that I would be completing.

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INTRODUCTION

My type of thinking is not wanted in this present age, I have to swim so strongly against the tide. Perhaps in a hundred years people will really want what I am writing.

— Ludwig Wittgenstein, 1949

This dissertation contains three papers written between 1993 and 1996. As I see them, the papers might be described as an attempt to explore what the field of organizational analysis might look like were it to take seriously the ideas put forth in Wittgenstein's (1958) *Philosophical Investigations*, as well as the ideas of those influenced by the latter. And just as Wittgenstein's text was a series of investigations of so-called philosophical problems, this text contains three investigations of what we might think of as 'organizational problems'. So it is for this reason that I have entitled it *Organizational Investigations*.

As the papers themselves describe in greater detail, what Wittgenstein sought to do was approach language, not as a mirror through which the world's reflection becomes visible to us (Rorty, 1979) but, rather, as simply some sounds and markings we emit in negotiating and carrying out particular social practices (Davidson, 1984; Rorty, 1992). And in doing so, he showed us that it is not correspondence with the world that makes a statement true but, instead, the fact that it is an agreed upon way of speaking. From Wittgenstein's perspective, when we agree that a statement is correct we are acknowledging that language has been used in the agreed upon manner. What we are *not* acknowledging is that our statement actually describes that which is the case. And when we accept this latter revelation, knowledge – and therefore science – come to be viewed in a very different light.

The papers contained herein are aimed at rendering the science of organization studies in this new light. The intention is not to propose a particular theory of a specific organization phenomenon, nor is the intention to carry out a literature review of any sort. Instead, my intention herein is to sketch out what our discipline might look like were we

to incorporate the later Wittgenstein's "discursive turn" within our investigations. As I describe it in Chapter Three, my intention is to guide our discipline through the transition from correspondence to coherence conceptions of truth. Either way, it would be a mistake to think that the contribution of these papers is to be found in better ways of organizing or improved corporate performance. Rather, the contribution is to be found in the possibilities for future research that the ideas presented herein make possible.

So to some degree, the three papers comprising this dissertation do not represent three distinct ideas for organizational research. Instead, what the three papers really amount to is the same message being delivered to three different audiences. And much like the politician who massages her or his message to suit the audience being spoken to, what I try to do in each of these three papers is couch my message in the vernacular of the particular group I am addressing. In more literal terms, what I try to do is anchor my message in the literature and issues which define the particular audiences I am addressing. By doing so, the hope is that each group will find my message more palatable than if I were to use the same vocabulary and literature in every case.

In the first paper, "Understanding language games", written with Nelson Phillips, the audience I have in mind is those who research macro-organizational phenomena, those who comprise the field of organization theory (OT). And in this particular case, the intention is to address a paper by Astley and Zammuto (1992; cf. Beyer, 1992; Donaldson, 1992) which introduced many in OT to Wittgenstein's concept of 'language games'. Specifically, we argue that the concept is considerably more nuanced, and its implications considerably more profound, than is indicated by Astley and Zammuto's treatment of it. Indeed, for we argue that organizational life can be usefully understood as being comprised of complex networks of 'language games' (Lyotard, 1984). In addition, we argue that, properly understood, 'language games' serve as a nodal point through which the literatures of other social sciences, as well as the humanities, might be connected with

organizational research. In brief, we argue that 'language games' can contribute a great deal more to our investigations than what Astley and Zammuto (1992) ask of it.

In light of its audience's seeming aversion to theoretical discussions and interdisciplinary investigations, the second paper, "The subject of organizational behavior", is perhaps the most ambitious of the three. But even for those inclined to theorizing, the argument presented therein may be somewhat unsettling. For what I do in this chapter is, first of all, show how the move to an anti-representationalist notion of language robs us of the possibility of claiming that the 'self' is, as the cognitive researchers are wont to argue, an "information processor". Furthermore, what I go on to argue is that this notion of language leaves us no way of showing that there is even a 'self' to get to know. Instead, what it leaves us with is the suggestion that 'self' is little more than an agreed upon way of speaking. So to those cognitive researchers who believe they might eventually reveal the "true" workings of the mind, the discussion in Chapter Two will be far from appealing.

But I said above that the intention with this dissertation is to suggest what our discipline might look like were it to actually make the 'discursive turn'. So in this regard, what I also do in the second paper is suggest an alternative to this 'cognitive self' that has dominated research in OB in recent years. Specifically, I introduce Pierre Bourdieu's (1977; 1985; 1990) notion of *habitus* and its connections with Wittgenstein's discussions of language. In brief, what I suggest is that it is the organizational habitus rather than the 'individual' or 'self' that those interested in organizational behavior should be exploring.

The third paper, "Relationality", is addressed to those who are interested in the metatheory of organization studies. But as such, its argument pertains to all who conduct organizational research, regardless of whether or not they might be interested in metatheoretical discussions. In very simple terms, what I do in this paper is problematize the Subjectivism/Objectivism dichotomy with which Burrell and Morgan (1979), among

others, have divvied up social science research. I suggest that, rather than representing two paradigms, the separation of research along this fault line is, in fact, indicative of a single paradigm, which I refer to here as the Duality paradigm. And if there is to be an "alternative" paradigm, I suggest that it is to be found in what I call the Relationality paradigm, which amounts to an amalgamation of the writings of Richard Rorty (1989), Pierre Bourdieu (1990), and others that have been greatly influenced by Wittgenstein.

To a large degree, the three papers contained herein represent a *deconstruction* of our traditional ways of approaching organizational phenomena. They seek to argue that we can take neither the subject of organizational behavior nor the object of organization theory for granted. In addition, they seek to establish that our scientific endeavors are, ultimately, social endeavors. They argue that we are not discovering a world of organizational phenomena so much as we are contributing toward the construction of a socially organized world. And in making this explicit, these papers point toward an increased responsibility to consider the moral implications of what we do as organizational researchers. For when we can no longer claim that we are objective observers "discovering" that which already exists, we have no alternative but to accept our complicity in the production of that which is to be in the future.

In light of this new-found moral responsibility, what I try to establish in the concluding chapter of this dissertation is the possibility of and the need for a *reconstruction* of our discipline. Contrary to those who invoke the discursive turn in arguing for an "anything goes" mentality, or even a neo-conservative agenda, following Bauman (1993), I argue that the discursive turn points instead to the need to construct our social edifices in relation to something "beyond" knowledge or, in Lévinas's (1981) terms, something that is "otherwise than being". In contrast with those who might see the deconstructionist tactics deployed herein as leading, at best, toward neo-conservatism, and, at worst, toward nihilism, the note I try to conclude on is that these tactics can, as well, contribute toward

the emancipation of the 'moral self'. Needless to say, it is toward this end that I hope the discussions contained herein will contribute.

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Chapter One

UNDERSTANDING LANGUAGE GAMES

Michael K. Mauws

and

Nelson Phillips¹

¹ In December of 1992 Nelson Phillips and I received our copies of *Organization Science* 3(4) which contained a series of articles having to do with Wittgenstein's concept of 'language games'. Nelson, knowing that I was quite interested in this area and knowing that I was like him disappointed in the articles, suggested that we write a response and submit it to the 1993 ASAC conference. This we did in the first week of January 1993 and, by the end of January, we had also submitted a more polished version to *Organization Science*. The resultant reviews were generally quite positive and, on this basis, Peter Frost asked that we develop the article into something more than just a response. This we did and, in 1995, the article appeared in *OS*. This chapter is, almost verbatim, that article.

If cognitive space could be projected upon the city map, or upon the map of a country or the modern world as a whole, it would take the shape of an archipelago, rather than a circle or any other compact and continuous figure.

- Zygmunt Bauman, *Postmodern Ethics*

In a recent paper Astley & Zammuto (1992) used the concept 'language game' (Wittgenstein, 1958) to explore the relationship between organization science and management practice. Accompanying this article were replies by Donaldson (1992) and Beyer (1992), both of whom were unconvinced of the value of Astley and Zammuto's argument and of the 'language game' concept in general. We believe, however, that an important source of the disagreement among these authors can be traced to the underdeveloped conception of language games with which all are working. For it is this that leads to the resulting misunderstanding of its usefulness for analyzing the contribution of organization science to managerial practice. And, similarly, it is this that obstructs a full consideration of the 'language game' concept's possible contribution to the discipline of organization science more generally. In other words, while we agree with Astley and Zammuto that the 'language game' concept can be useful in understanding the relations among scholars and practitioners, we also believe that a more complex and nuanced conceptualization allows for its extension to a number of other significant issues that are of concern to organizational researchers.

Given the increasing recognition of the centrality of language to the phenomenon of organization (e.g. Boje, 1991; Kilduff, 1993; Sandelands & Drazin, 1989; cf. Sköldbberg, 1992), we believe that the notion of 'language games' is sufficiently valuable to warrant a more comprehensive review. In addition to providing a means for understanding the relationships among organizational scientists and management practitioners, the concept provides an important stepping off point for investigating a number of other organizational issues. Most importantly, it provides a valuable bridge to a range of theoretical perspectives not often used in organizational science such as deconstruction (e.g., Derrida,

1978; Norris, 1991), post-structuralism (e.g., Dews, 1987), and the various strands of post-modernism (e.g., Jameson, 1991; Lyotard, 1984), that are only beginning to garner interest within the discipline (e.g., Calás & Smircich, 1991; Cooper & Burrell, 1988; Kilduff, 1993).

It is important to note, however, that our intention with this article is not to suggest that there is one correct definition of "language games". Nor is it to suggest that there is one correct reading of Wittgenstein's work. Rather, our intention is to point out that, while the term "language game" can be used in many different ways, some understandings of the term are more powerful than others; i.e., like all concepts, the usefulness of the 'language game' concept is dependent upon how it is understood. This said, Wittgenstein's discussion of the concept is the most developed and powerful we know. So our aim, then, is not to argue that others' definitions are wrong, but to encourage our reader to adopt our definition of "language games" - to join our language game if you will - by pointing to the weaknesses of the definitions adopted in the previous dialogue. In this manner we hope to illuminate the strengths of our own conception of language games - one which we believe is arguably closer to the original concept as it was presented by Wittgenstein (1958).

That which follows is divided into four sections. In the first we review Astley and Zammuto's conception of language games and their application of this concept to the relationship between organization science and managerial practice. The second section presents our reading of Wittgenstein's (1958) discussion of language games which we then use to extend and critique Astley and Zammuto's argument. Using this more developed conception, in the third section, we examine the arguments of Donaldson (1992) and Beyer (1992) and the manner in which their differing understandings of 'language games' diverts them from a fuller engagement with the arguments of Astley and Zammuto. In

the final section, then, we collect together and discuss the various ways the 'language game' concept can contribute to organization science.

LANGUAGE GAMES: ASTLEY AND ZAMMUTO

Astley and Zammuto's (1992: 444) interpretation of the 'language game' concept is summed up in the following statement:

According to Wittgenstein, words derive their meaning not from the actions or objects that they denote, but from the historical context of discourse, or language game, in which they are used. The meaning of words is specified by rules of intelligibility embedded in the institutional context in which language is employed. Linguistic interpretations of phenomena and events are always grounded in prior sets of socially defined understandings about the nature of reality. These understandings are conveyed in the stylized vocabularies and protocols of communication that comprise language games.

In using this concept, Astley and Zammuto wish to draw our attention to the "specialized forms of discourse engaged in by members of a particular community - in this case by members of the organization science and managerial communities" (1992: 444). These two communities are "semi-autonomous domains" each of whose discourse is characterized by its own rule-system. But despite their reference to the same underlying phenomena, the "divergent foci and interests" of the two language games cause them to possess "distinctive, [and] sometimes incommensurate, terminologies." For this reason, the meaning of particular words and concepts can only be determined in reference to the particular language games in which they are being deployed. It is the status of organization science and managerial practice as separate language games that defines them and that limits communication between them.

Astley and Zammuto discuss several aspects of the 'language game' concept which they feel to be useful to the argument they are constructing. Most notably, the assumption is made that perception - and presumably thought - is unavoidably situated in language; i.e., our knowledge of the world must be understood as a product of language

rather than the unencumbered product of empirical observation. Thus, for Astley and Zammuto the meanings of words are not in a one-to-one relationship with the objects and actions they denote in the observable world; instead, meanings are determined through the historical development of specific language games in which the usage of particular words constitute valid moves in the game. It is only within the situated discourse of a particular language game that an act of meaning can take place (Bruner, 1992).

Beginning with this conception of language games, Astley and Zammuto go on to explain how the knowledge produced by the organization science community can contribute to the practical activities of the managerial community. They base their argument around Pelz's (cited in Astley & Zammuto, 1992) threefold typology of knowledge utilization - the instrumental, conceptual and symbolic modes of application of social science - combined with the idea developed above of organizational science and managerial practice as separate language games. The instrumental mode of application emphasizes the development of techniques, tools, and practices that can directly impact managerial action (Astley & Zammuto, 1992: 454). This is generally the kind of knowledge that critics have in mind when they argue that organization science is simply not relevant to managerial practice. The difficulty, however, is that the distance between the language game that characterizes organization science and the language game that characterizes management practice is simply too great to allow organizational researchers to produce tools and techniques that are actually useful to managers. In the words of Astley and Zammuto (1992: 453), "even if researchers began conducting more applied research, the potential instrumental impact is not likely to be high because the further the researcher's location from the organizational context being studied, the more difficult it is to transform research results into direct action."

This does not mean that organization science cannot contribute to management practice. Rather, for Astley and Zammuto one of the most important contributions of

academic discourse to the activities of practitioners is the provision of alternative conceptualizations of managerial problems. That is, as a result of their differing language games, the concepts and theories which academics invoke to explain organizational reality are considerably different from those of practitioners: "we supply concepts, and these alter perceptions" (Cronbach, cited in Astley & Zammuto, 1992: 453). The distance between the two language games, however, means that this is only possible because of the inherent ambiguity of academic theories: "What highly general theories lose in terms of specific empirical content ... they gain in terms of meaning" (Astley & Zammuto, 1992: 445). If they choose to strive for empirical precision - if they attempt to produce instrumental knowledge - researchers lose the broad applicability that makes organizational research truly useful to managers. Managers must be able to take concepts and apply them to their specific situation, one which in all likelihood has little to do with the context in which the research took place and one which varies along dimensions unimaginable to the researcher. For this to occur, theories must be broad and flexible; they must inspire managers; and they must be conceptually rich - all of which are characteristics that prevent their application in a straightforward, instrumental way to organizational problems.

Organizational researchers can also contribute to management practice in another way. Because the two communities can be considered autonomous language games, the findings of organization science can serve to legitimate managerial actions. In other words, as a result of the separation of organization science and managerial practice, their mutual agreement on a course of action establishes its appropriateness through a process of triangulation. Managers can therefore produce convincing accounts for their actions using the findings and theories of organizational science. In the absence of the separation between the language games - in the absence of the putative "objectivity" of organizational science - the organization science community would no longer be able to provide the

legitimation which management requires to gain support for potentially controversial activities.

Organizational researchers are therefore left with a choice. They can attempt to produce knowledge that can be applied in an instrumental fashion but that will, in all probability, never be very successful. Or they can work to produce knowledge that can be applied in a conceptual or legitimating mode capitalizing on the distance between the managerial and organization science language games. The kind of knowledge that is most useful in each case is very different: abstract and ambiguous knowledge that can be applied conceptually or used to legitimate action *versus* specific and empirically testable knowledge that may have limited instrumental applications. For Astley and Zammuto the choice is clear: the field should focus more directly on developing broad conceptual frameworks that practitioners can use to understand their world and challenge their favored ways of thinking: "Organization science may thus facilitate practice more through process than content - instead of discovering empirically derived solutions to specific problems, it provides conceptual language that shapes managers' perceptions and thoughts, thereby enhancing their problem-solving capabilities" (Astley & Zammuto, 1992: 455).

LANGUAGE GAMES: WITTGENSTEIN

While Astley and Zammuto (1992) have identified some important dimensions of the 'language game' concept, we feel that there are several areas in which they are unclear or imprecise in their presentation. In addition, there are also areas where the ramifications of the 'language game' concept are not sufficiently developed. These weaknesses in their discussion limit the applicability and usefulness of the concept and leave theoretical gaps that are capitalized on by their critics. So in this section we modify and extend aspects of the 'language game' concept in order to close those gaps and broaden its applicability.

Reality as a Social Product

Wittgenstein's conception of a language game grew out of his interest in the relationship between language and reality (Hanfling, 1989: 130; Harris, 1988); i.e., how are objects and actions in the world connected to our understandings of them?² In asking this question Wittgenstein moved beyond his initial interest in simple propositions in *Tractatus Logico Philosophicus* (1922/1990) and began to consider language in use. In his later work, and particularly in *Philosophical Investigations* (1958), Wittgenstein came to see language as more than simply a medium of communication: "Here the term "language-game" is meant to bring into prominence the fact that the speaking of language is part of an activity, or a form of life" (Wittgenstein, 1958: 11; cf. Lyotard, 1984). Language is not some sort of pre-existing system that we uncover, but rather a product of our ongoing interaction (Rorty, 1989: Ch. 1). Through our interaction we develop particular modes of communication which reveal a world to us and which provide particular tools, concepts and practices which further facilitate interaction. And for Wittgenstein, the limits upon the kinds of tools and concepts are very few:

But how many kinds of sentences are there? Say assertion, question, and command? - There are countless kinds: countless different kinds of use of what we call "symbols", "words", "sentences". And this multiplicity is not something fixed, given once for all; but new kinds of language, new language-games, as we may say, come into existence, and others become obsolete and get forgotten. (Wittgenstein, 1958: 11)

In other words, each language game produces a particular form of life; our participation in a particular language game is fundamentally constitutive of the "reality" within which we find ourselves (e.g., Foucault, 1978).

² An alternative introduction to Wittgenstein's ideas can be found in Derek Jarman's recent film, *Wittgenstein*, written with the assistance of Terry Eagleton (1993). This highly abstract and stylized movie draws a great deal of its dialogue from Wittgenstein's writings and takes full advantage of the visual element in exploring the relationship between language and reality.

Seen in this light, Wittgenstein's conception of language games carries with it a significant challenge to correspondence, or nomenclaturist, theories of language (Harris, 1988; Rorty, 1979). According to the latter, a statement is true when it reflects reality; when it corresponds to what is actually in the world waiting to be discovered. In science, this notion of language translates into an empiricist conception wherein the world is "out there" and theories are true when they are shown to correspond to that world. Since truth exists "out there", it is available, if approached correctly, to discipline scientific theory (Popper, 1959). The goal of science in this view is to develop a language and a set of theories which map the world as it really is; to discover the world, as Donaldson says, in its "actuality" (1992: 465).

But the concept of 'language games' challenges this conception and posits, instead, a theory of the world in which there is no unmediated access, no language which allows us access to what is really "out there" (Winch, 1958: 15). As described by Astley and Zammuto (1992: 445), "language is not a neutral medium for communicating facts; it determines the epistemological constitution of those facts in the first place." And this is a critical point. From a 'language games' perspective, there is not one empirical world to be discovered but rather as many worlds as there are language games (Astley, 1985; Feyerabend, 1988; Gergen, 1985). Each language game provides a language with particular meanings. This language produces a particular kind of world in which certain objects - including the speaking subjects - are made to exist in certain relations. But from this perspective no *definitive* world exists, only the plural worlds created by the language games which exist at any particular time. And while falsehood is clearly possible - when the world as revealed by a particular language game fails the tests of truth *associated with that game* - the 'real' world is not available to discipline our theories in an empiricist sense:

"So you are saying that human agreement decides what is true and false?" - It is what human beings say that is true and false; and they agree in the language they use. That is not agreement in opinions but in form of life (Wittgenstein, 1958: 88).

For if we follow Logic to its teleological end, it is only statements and propositions that can be true or false; the world itself simply is (Merleau-Ponty, 1964; Rorty, 1989).

So the idea of testing language games on the basis of a fit with the world loses much of its sense when one realizes that there is no external, Archimedian point from which to judge these games - no extramundane point outside of all language games from which we may make an disinterested decision. Instead we are left with the conclusion that truth only exists within language games, that language games are human creations, and that the particular criteria of truth (e.g., logical consistency, significance levels, etc.) extant within a particular language game - and truth itself for that matter - are also human creations (Lyotard, 1984). In this scenario, the idea of a description of the world has been moved from the level of a statement or hypothesis to the level of an entire language game.

So what does this mean for science? First of all, it means that there are no objective or external criteria on which to judge the relative worth of scientific language games. There is no external position - no transcendental criteria of truth - from which to judge the relative worth of a particular scientific perspective. As Wittgenstein (1958: 96) describes it, "Imagine someone saying: 'But I know how tall I am!' and laying his hand on top of his head to prove it." Instead, each language game defines a language - a set of practices - and, most importantly, a set of criteria for truth.

Different games can, therefore, say little about the validity of other games other than to invite members of those other game to join theirs, perhaps until no one is left playing the other game. This sometimes happens in science, as both Kuhn (1970) and Feyerabend (1988) have chronicled in their respective histories of physics. But when it does, it is not the world showing through but is, instead, some set of social forces which drive players from one game to another:

It is clear that the allegiance to the new ideas will have to be brought about by irrational means such as propaganda, emotion, ad hoc hypotheses, and appeals to prejudices of all kinds. We need these 'irrational means' in order to uphold what is nothing but a blind faith until

we have found the auxiliary sciences, the facts, the arguments that turn the faith into sound 'knowledge' (Feyerabend, 1988: 119).

In other words, until a language game of sufficient complexity has developed, there are no criteria by which to judge its success since the criteria for 'truth' within a language game are a part of the rules of that game. For without the active intervention of social forces, no new scientific language game would ever begin. New theories require new empirical tests and new criteria of truth. Until these are developed it is social forces which sustain these new theoretical perspectives, these new language games.

Games and Meta-Games

In their application of the 'language game' concept Astley and Zammuto present organization science and managerial practice as different language games. These language games, they suggest, are large cohesive entities possessing sets of characteristics that serve to differentiate them despite their interest in a common phenomenon. But there is a significant problem with this train of thought in that it is not entirely clear that Wittgenstein's concept of 'language games' can be sensibly applied to a group of practices as broad and diverse as those included in either 'organization science' or 'managerial practice'. For when we re-examine Wittgenstein's (1958: 11-12) conception of language games we find examples such as the following:

- Describing the appearance of an object, or giving its measurements—
- Constructing an object from a description (a drawing)—
- Reporting an event—
- Speculating about an event—
- Forming and testing a hypothesis—
- Presenting the results of an experiment in tables and diagrams—
- Making up a story; and reading it—
- Play-acting—
- Singing catches—
- Guessing riddles—
- Making a joke; telling it—
- Solving a problem in practical arithmetic—
- Translating from one language into another—
- Asking, thanking, cursing, greeting, praying.

And these examples appear to have very little in common with organization science or managerial practice in the abstract sense of Astley and Zammuto's (1992) discussion. Instead of a large and amorphous mass of activities, we find as examples simple, well-defined acts. The examples are all purposeful, rule-governed, and bounded in time, space, and duration. So it may be, then, that the 'language game' concept - at least Wittgenstein's conception of it - is stretched past the breaking point when applied to an entire academic field or to the range of activities encompassed by managerial practice. In other words, it may be that Wittgenstein's objective was to understand instances of linguistic activity that occurred on a small scale for a particular purpose; thus, his concept may have correspondingly little to say, at least directly, about an entity as abstract as organization science or managerial practice.

But rather than dismissing the 'language game' concept as irrelevant to the particular problem at hand, we would like to argue that it is difficult to imagine any conceptualization that could support the identification of organization science and management practice as discrete and cohesive entities while still maintaining a separation between the two. Simply stated: on what basis could we justify such a distinction? For example, how might we combine the entire range of research, teaching, and consulting activities carried out by the organization science community while separating it from the activities of all managers? Aren't many of the activities carried out by managers much like those carried out by researchers? Don't managers gather information, analyze it, and produce reports on the results? Don't managers think about organizations and share the results of their considerations? And what does it mean to suggest that both communities share the same object of interest? Do they? That is, does this object even exist outside of the language game in which it has been, and continues to be, constructed? In other words, while these communities of individuals may have practical and political reasons for

understanding themselves as separate, and perhaps cohesive, there is not a correspondingly clear division to be found in the particular language games each group participates in.

So is the language game concept at all useful in understanding the nature of these communities and their relation to one another? We believe it is. For while organization science and managerial practice share few characteristics with the examples of language games cited above, they are not completely divorced from them either. If we move from the highly abstract and general perspective of Astley and Zammuto to the micro-scale of Wittgenstein the relationship becomes clearer. At this level, groups of researchers work on specific projects, write papers, and present "the results of an experiment in tables and diagrams" (Wittgenstein, 1958: 12). Similarly, groups of managers run particular organizations, cooperate in industry groups, and compete in markets. Instead of language games, organization science and managerial practice are better understood as collections of diverse and unconnected language games - what Lyotard refers to as "flexible networks of language games" (1984: 17). As networks they are like a human brain, each consisting of a multitude of neurons - language games - connected in numerous ways: by similarity of purpose, by their antagonisms, and especially by the participation of the same individual across different language games. Extending the metaphor, organization science and managerial practice can be thought of as the two hemispheres of the brain, each consisting of a complex network and each connected to the other hemisphere in a number of complex ways.

Paradigms and Language Games

Astley and Zammuto's over-generalization, or perhaps under-definition, of the 'language game' concept has other ramifications. One is its association with the Kuhnian notion of paradigms:

Such considerations have led some observers to draw a parallel between Wittgenstein's conception of a language game and Kuhn's (1970) notion of a paradigm. Kuhn argued that

prevailing views of the truth are the result of paradigmatic interpretations - they reflect accepted ways of looking at the world embodied in dominant theoretical perspectives (Astley & Zammuto, 1992: 445).

But while paradigms may, in some contexts, be usefully understood as language games of a particular kind - at least to the degree that they match the kinds of characteristics noted above - not all language can be considered paradigms. For example, the idea of 'paradigms' has little to say about guessing riddles or translating from one language to another. On the other hand, the concept of 'language games' has very little to say about paradigms when the concept of 'paradigms' is generalized away from Kuhn's initial notion of working communities of scientists and their shared concepts, skills, and practices; i.e., researchers "whose research is based on shared paradigms are committed to the same rules and standards for scientific practice" (1970: 11). For understood in this way, it is difficult to see organization science, and even more so managerial practice, as paradigms.

The concept of 'language games' and the moves that are a part of them - at least as presented by Wittgenstein (1958) - is fundamentally oriented toward understanding *language in use*. Language games are activities that we *participate* in with each and every act of communication. Thus understood, the concept has little concern, if any, with the world - except in those language games that explicitly appeal to something called "the world". Paradigms, on the other hand - at least in Kuhn's (1970) sense - are fundamentally about the world; the various communities associated with particular paradigms are only separable *because of* their differing conceptions of the world. Thus, depending upon the particular language game at hand, the participants may or may not be associated with differing paradigms, but it is only within a small set of games that claiming affinity with a 'paradigm' is considered to be a valid move. For example, within the language game of "singing catches", the notion of 'paradigm' makes little sense, whereas it is often central to discussions of ontology. Conversely, adherents to particular 'paradigms' participate in a multitude of language games, many of which have nothing to

do with their particular paradigm - for example, Ptolemy's and Copernicus's differing astrological paradigms would not, presumably, have prevented them from "singing catches" together.

Both 'paradigm' and 'language game' can be useful concepts for understanding the relationship between organizational scientists and management practitioners. Each, however, provides for a different means of understanding this relationship (Rao & Pasmore, 1989). We can, using Kuhn's (1970) notion of 'paradigm', attempt to argue that scientists and practitioners understand the world in fundamentally different ways that obstructs dialogue - that are "incommensurable", to use Kuhn's terminology. Alternatively, using Wittgenstein's (1958) concept of 'language games' we can explore the more micro activities that these two communities participate in to see if it is here where the connections between the two are to be found. And should we choose to rename Wittgenstein's concept "paradigms" or rename Kuhn's "language games", the underlying concepts which these authors are seeking to explicate will continue to have significant differences between them regardless of the labels attached to them. So to argue that the concepts referred to by Kuhn's use of "paradigm" and Wittgenstein's use of "language games" are one and the same is to do damage to both. Both could have coined a new term to identify the phenomenon in which they were interested but chose not to, presumably to capitalize on some of the connotations associated with the terms they selected. That they did so, however, does not imply that any and all connotations be attributed to their respective discussions.

Again, the question here is not that of getting the definition of "language games" or "paradigms" right. Rather it is a question of using the most powerful *conceptualization* of a particular term and, in this instance, what we feel to be the *intended* conceptualization of the term. Here, Astley and Zammuto wish to invoke both Wittgenstein and Kuhn, names which presumably carry with them some legitimacy. But shouldn't that also carry with it

some responsibility to be accurate in their use of the term? Shouldn't any significant departures from the original usage be explained and justified? Although there are similarities to be found between Kuhn's (1970) notion of 'paradigm' and Wittgenstein's (1958) notion of 'language games', there is as much separating them as uniting them. In fact, what characterizes Astley and Zammuto's presentation of both 'paradigm' and 'language game' is an over-generalization to a point where almost any interpretive social theory could be so defined and used interchangeably. Berger and Luckmann's (1966) social constructivism, Schutz's (1970) phenomenology, Geertz's (1973) semiotic theory of culture, or Mead's symbolic interactionism (1962), could all be similarly defined in one ambiguous or abstract paragraph and used to produce a wonderfully pluralistic, but equally vague, and perhaps unfounded, position. Furthermore, as we attempt to illustrate in the following section, it is precisely Astley and Zammuto's under-definition of their key concept that allows their critics to avoid addressing their argument fully.

LANGUAGE GAMES: CRITIQUING THE CRITICS

Donaldson: The Weick Stuff

Our primary difficulties with Donaldson's article "The Weick stuff: managing beyond games" stem from his equation of 'language games' with 'antipositivism' (cf. Rao & Pasmore, 1989) and his insistence that we must study "the actuality of organizational management" (Donaldson, 1992: 465). Although we fully agree with him in noting that anti-positivism might be considered as a language game within Wittgenstein's theoretical framework, we also assert that, within this framework, positivism must also be addressed in similar fashion. We also disagree with his assertion that there is a single 'actuality' to study, suggesting instead that our research methods and theoretical positions condition what we find when we approach the 'actualities' of organizations. Thus, in critiquing

Donaldson we are once again attempting to clarify what it is that is meant by the concept of 'language game'.

As was already mentioned, the concept of a 'language game' was developed by Wittgenstein to account for the relation between language and the world. It is because the world does not naturally divide itself into facts and objects with preordained names that this relation must be addressed (Jameson, 1990; Merleau-Ponty, 1964; Rorty, 1989). What 'language games' helps us to understand is that it is only through the rules and conventions established through social interaction that it is possible to speak of the things that are in the world. Therefore, as conceptualized by Wittgenstein (1958), meaningful statements cannot be made outside of a language game³, and statements within both positivism and anti-positivism, because they presuppose a system of rules, are understood from this perspective as moves within their respective language games. Donaldson, however, associates language games only with anti-positivism and, therefore, fails to see the manner in which positivism and anti-positivism are both understood to be language games within this framework.

Donaldson asserts that the concept of 'language games', at least as it is used by Astley and Zammuto, is specifically opposed to the agenda of positivism, which he summarizes in following manner:

practical action is to be served by the construction and validation of theoretical models that state in language that has operational implications which approach is optimal in any specific situation given the policy and value objectives (Donaldson, 1992: 461).

Thus stated, he goes on to say that:

Like the rest of antipositivism, the tendency [with language games] is to undermine existing scientific approaches in the study of organization and thereby to move the focus away from

³ The development of this position raises the question of private languages and whether these would include rules. The position which Wittgenstein (1958) develops at some length is that private languages are not possible.

the study of actuality in organizational management and towards what can only be called language games (1992: 461).

What the 'language game' concept attempts to reveal, however, is that it is only because of the associated rule system which constitutes their game that the members of the positivistic community can "construct", "validate" and "operationalize" the theories they require to carry out their "practical action". It is only in this sense, as a rule-governed language game, that positivism can be said to exist. Thus, contrary to Donaldson's (1992: 461) assertions, the aim in invoking 'language games' is not "to pry loose the connection between theory and data"; nor is it "to open the way for organizational theories to be accepted as metaphors floating free of specific referents to concrete organizational features" (although the latter would constitute language games of yet another sort). Instead, the concept of 'language games' is meant to provide a theory of meanings, which may or may not have a concrete referents in the world.

However, in contrast to his initial resistance, Donaldson (1992: 461-2) later seems willing to provisionally accept the "language game theory of meaning" in an attempt to argue the positivist cause. In this regard, we are wholly in agreement with his references to "protocols of observation", "communally understood behavior sequences", and mutually agreed upon statistics as examples of the rules by which the positivist language game is played. In addition, we fully support his assertion that "the Wittgensteinian language doctrine that meaning is usage still allows the positivistic conception of social science to be meaningful." However, where we fundamentally differ from Donaldson is that we see *all* conceptions of science as meaningful; the relevant question here being, for whom (Weick, 1992)? But because we are unable to conceive of an epistemological vantage point from which positivism could assert its privileged access to reality, we can see no philosophical basis to universalize its game and to exclude all other games which portray the concrete referents that are the world.

Ultimately, though, our point of departure with Donaldson occurs at the ontological and epistemological levels. The ontological position which he espouses is of the "one world - one reality" variety and is diametrically opposed to the "one world - multiple realities" (see Marsden, 1993) understanding that informs Wittgenstein's development of the language game concept. There is no "actuality in organizational management", there are just the many actualities revealed by the different language games of organization science, of management, and of other kinds of games such as journalism, film, theatre, talk shows, soap operas and narrative fiction. Although Donaldson seems to recognize the problem of determining meanings that 'language games' seeks to address, he fails to acknowledge the full ramifications of adopting this position. He believes that meanings are not contested, or, if they are, that their differences are resolvable through recourse to some arbiter outside of the network of language games. It is in this sense, then, that we have epistemological difficulty with his position. Thus, through our reading of Wittgenstein and those he has influenced (e.g. Lyotard, 1984; Winch, 1958), we find it difficult to sustain either the ontological or epistemological assumptions from which Donaldson's critique has been launched.

To conclude, the concept of 'language games' says nothing directly about how organizational researchers should carry out their work; it favors neither positivism nor anti-positivism. What it provides is a theory of language in use that sets limits on the kinds of things we can know and explains some aspects of the relation between different approaches to research. It is not contrary to positivism in any direct sense; it simply points to the limitations of the game and to some problems of applying natural science models to social science. It asks organizational researchers to be more sensitive to questions of meaning and, perhaps, to be more humble about the generalizability of their knowledge. It also demands constant justification for methods of all kinds; it points to the tendency for

members of a language game to set the rules of the game and then to understand those rules as natural laws.

Beyer: Metaphors, Misunderstandings, and Mischief

In her critique of Astley and Zammuto's (1992) argument, "Metaphors, misunderstandings, and mischief: a commentary", Beyer (1992: 468) reduces Wittgenstein's complex and well-developed concept of 'language games' to the level of "an indirect metaphor - somewhat like a literary allusion" - and then proceeds to evaluate its contribution by whether it "refer[s] to something so familiar to readers that it immediately evoke[s] a whole complex of meanings to them". And although we do not deny the metaphorical possibilities of the term "language game", we believe that Wittgenstein intended this term as considerably more than a simple metaphor whose connotations might be settled through recourse to the "unabridged Webster's dictionary" (Beyer, 1992: 469). Treating "language games" in the sense that Beyer has would imply that we similarly treat other such well-developed organizational concepts as 'culture' and 'leadership' solely in terms of their dictionary definitions and metaphorical implications. But even if Beyer were willing to concede this, the fact remains that her understanding of the 'language game' concept is fundamentally different from that of Wittgenstein (1958). This difference, however, serves to illustrate the varying outcomes which can accompany the same move - the use of the same term - made within differing language games.

Why Beyer (1992: 468) should assume the authors only meant to convey "some of the meanings Wittgenstein (1953) intended by his use of the term" eludes us. Although we have argued above that Astley and Zammuto (1992) do not make full use of this concept, we can find no reason to suspect that this was their intention. Invoking the concept in the manner that they do, we understand the authors to be drawing upon the full discussion of 'language games' which Wittgenstein presents in his *Philosophical Investigations*. Had the

authors chosen to analyze the academic and practitioner domains as distinct 'cultures' instead, and then cited Trice and Beyer (1993), we would, similarly, presume that they intended to convey all the subtleties of culture which the latter conveyed. Beyer (1992: 469), however, rather than working within her own brief explication of Wittgenstein's position, seems to prefer the even more succinct definitions of "Webster's unabridged". Unfortunately, given that bounded meanings such as those in dictionaries are one of the fundamental issues which the concept of 'language games' seeks to problematize, it is evident that Beyer has not fully comprehended Wittgenstein's position.

But Beyer does, in her use of a dictionary, provide a small glimmer of hope in acknowledging that the notion of games as "physical or mental competitions conducted according to rules in which the participants play in direct opposition to each other" (Gove, cited in Beyer, 1992: 469) is perhaps closest to the usage which Astley and Zammuto (1992) wish to make of it. This hope quickly disappears, however, when she somehow construes this definition as being negative in its connotations. But it is exactly this sense of "competition" and "opposition" in the presence of "rules" that we feel Wittgenstein was trying to convey through his use of this concept (Lyotard, 1984). Contrary to Kirk and Miller (cited in Beyer, 1992: 470), and in keeping with our discussion of a socially constructed reality above, the notion of 'language games' is fundamentally opposed to the idea of "calling things by their right names". It is, instead, about the struggles which take place in determining what is right and what, for a time, may become fact. In this sense, Webster's definition might be rehabilitated to say that both science and business are "competitions conducted according to rules in which the participants play in direct opposition" to determine that which will become "Truth". The concept of language games sensitizes us to the fact that the "right names" are determined according to rules and these rules constitute a language game.

Beyer, then, falls short in her critique of Astley and Zammuto's usage of the 'language game' concept because she chooses to address the concept as metaphor rather than as the well-defined theoretical concept it was intended to be. To her credit, though, it may be that Beyer has chosen to formulate her critique in these terms in order to more directly confront the very general and ambiguous application which Astley and Zammuto make of this concept. However, that this is so only serves to underscore our position that the use of the 'language game' concept in an undeveloped form is more likely to serve as an impediment to understanding than to further it.

ORGANIZATION SCIENCE AND LANGUAGE GAMES

To this point we have focused on the language game concept and its use in the three papers discussed above. In our response to these papers, we have developed a conception of language games that draws more directly on the work of Wittgenstein and that is more useful in studying organizations and in understanding the relation of organization science to management practice. We will now extend Astley and Zammuto's discussion by drawing out more clearly the ramifications of accepting Wittgenstein's arguments for the study of organizations and for our understanding of organization science as a field.

Organization Science, Managers, and Language Games

From the perspective presented here, "organization science" and "management practice" do not correspond to two large, unified language games. Instead, each term denotes a complex, inter-related set of language games – what Lyotard (1984: 17) refers to as "flexible networks of language games" – whose unity is more *a function of the occupational groups that participate in them*, than of any coherence among them. Nevertheless, when discussing the activities of organizational scholars there is, virtually without exception, an attempt to wrest coherence from the fragmented and often

unrelated activities associated with participation in the organization science community. For example, the argument is often made that scholars carry out research on problems of concern to organizations and then disseminate their findings to students in the classroom, as well as applying them in their consulting activities. Thus, described in this way, organization science is reduced to the single, unified language game called "research" upon which accusations of irrelevance tend to be based (see Astley & Zammuto, 1992: 443).

When organization science is understood as a single language game in which academics, practitioners and students all participate, the debate over scholarly contribution to managerial practice is destined to result in a spectacle being made of seemingly overly-esoteric research. But when we recognize, instead, that what we call "organization science" is composed of a variety of language games that may be fragmented or connected in less than obvious ways, 'moves' such as that just described might be avoided. For example, few academics seek publication in peer-review journals such as *Organization Science*, *Administrative Science Quarterly*, or *Management Science* for the purpose of communicating with managers. Instead, publishing in these journals is a language game in which academics are the only real players and the reward structure is one in which only academics are likely to be interested. To seek the contribution of organization science to managerial practice in these journals is to have misunderstood the language game called "publishing". And, similarly, to dismiss all of organizational science on the basis of what appears in these journals is to mistake it for the large, unified language game that Astley and Zammuto have taken it to be.

Organization science, then, is not a single, integrated enterprise in which all activities contribute toward a single identifiable goal. Instead, academics participate in a number of unrelated games such as research, consulting, and teaching. But even these divisions are too broad to capture the diversity of activity and the concreteness of the actual practices. Research, for example, is a very broadly defined language game whose activities might be

further broken into the many other language games of which it is composed and to which it is connected. Results of research can be used to participate in the 'publication game'; the experience gained in carrying it out can assist in the 'classroom game'; and noting it on one's curriculum vitae can be a strategic 'move' in gaining work in the 'consulting game'. However, what must be noted here is that, when organization science is understood as a collection of language games rather than as a single unified game, practitioners are no longer seen as players within all of the games in which academics participate. Both groups undoubtedly participate within the 'consulting game' and, most importantly for this discussion, within the game of evaluating the contribution of organization science to managerial practice. But in large part, practitioners are only distant spectators to most of the games that scholars play.

Managers, then, may or may not be part of the particular games that management researchers play. And when they are, their role will vary from game to game and from time to time. In some instances, language games will be devoted to producing very specific instrumental knowledge for a single organization - the game we call "consulting" - while others will be actively producing knowledge useful only for the research community itself (methodology and philosophy of science discussions, for example). But all individuals are involved in many different games, allowing information and concepts to pass from game to game when and if they are deemed useful. Thus, the resulting model is not the two solitudes of Astley and Zammuto, but rather a flexible and interconnected network of language games; a model perhaps best understood as a complex and interwoven fabric of language games wherein each individual is a thread and language games are the knots that bind them together.

Organizational Research and the Language Game Concept

While the language game concept is helpful in understanding the relation between organizational science and managerial practice, it has other more significant ramifications for our discipline. Specifically, the idea of a language game can be applied directly to organizations as social accomplishments, resulting in the observation that what we - as 'organizational theorists' - call "organizations" are *de facto* groups of actors participating in inter-related sets of language games. In other words, the ongoing accomplishment of organization is a linguistic phenomenon and any particular organization will be a product of a complex history that has produced the rules and moves of the many language games that characterize it. Thus, organizations should not be approached as objects but as processes, as ongoing social accomplishments that are sustained through constant interaction. From a language game perspective, the knots in the fabric of organization are language games and the usage of a term (or gesture or practice) is mediated by the language game in which it occurs. Meanings, therefore, will vary within organizations - as well as from organization to organization - in unpredictable ways.

For this reason, organizational research must be extremely sensitive to the differences in usage from one organizational context to another. This would imply that organizational research should be heavily weighted toward methodologies that can deal with the different inter- and intra-organizational variations in usage. Organizational research that suppresses these differences through the restriction of variability in responses (experiments), through the forced selection of items (surveys and interviews), and through the gross coding of open-ended discourse (content analysis) must be carried out with great caution to avoid the suppression of variability between and across organizations (Potter & Wetherell, 1987). And although more structurally-oriented approaches remain a viable research strategy, great care must be taken in interpreting their results to avoid suppressing the variability of

usage across organizations and perhaps, as a consequence, lumping together very different things.

Good organizational research is therefore that which describes the practices and objects that characterize and reproduce particular organizations. From a language game perspective we must describe the rules and pieces that make up the game, the degree of flexibility allowed by the rules of the game, and kinds of statements and viewpoints that the rules of the game privilege. It is not that the language games *describe* the subjects, objects, and concepts that we tend to think of as being this or that 'organization' - although describing it in this manner is the language game we call "organizational research". Instead, it is through language games that these entities come into being (Deleuze, 1988; Foucault, 1972).

This perspective has profound consequences for organizational research: Following Wittgenstein, organizations - as language games - are forms of life. To a greater or lesser degree (an empirical question), different organizations are characterized by different approaches to what we call "the world" (Smircich & Stubbart, 1985). Organizational members do not experience organizational membership as an external aspect of their life, but rather live it and are shaped by their interactions with the people and objects which make up the organization. We must, therefore, deal carefully with the explanations provided by organizational members. The explanations given for organizational activity, no matter how intuitively appealing, are always accounts drawing on the resources of the language games in which they are situated. These accounts must be disassembled and used as to understand the dynamics of the language game rather than taken at face value as the reason for the existence of the game (Garfinkel, 1967, Ch. 8; Potter & Wetherell, 1987). Our participation in similar language games should not result in the assumption that we understand the organizational language that we are attempting to learn.

The language game concept, then, does not simply point to the importance of one kind of account or another; it points to the importance of language as the fundamental substrate on which organizations exist. The language game concept provides a useful approach to organizational research that sensitizes us to the fundamentally linguistic nature of organizational activity. In this regard, various strands of the organization science literature have investigated language use in organizations from various perspectives. But a direct focus on language and its importance in structuring organizations and organizational activities has been, in large part, missing (cf. Foucault, 1979; Gephart, 1978). Thus, the language game concept focuses our attention directly on the role of language in structuring organizational reality, making available various methodologies from other social sciences for analyzing language in organizations (e.g., Foucault, 1972; Parker, 1992). Thus, the concept of 'language games' helps provide the justification and legitimation for the inclusion and further utilization of these methods.

CONCLUSIONS

In this paper, we have focused on the concept of a language game and its use in three papers. The papers form an important dialogue on the nature of the field and provide three different perspectives on the language game concept. Astley and Zammuto (1992), the central paper in the dialogue, introduces the language game concept and applies it to the relationship between organization science and management practice. But the authors fail to develop the language game concept in sufficient detail to justify the demands they place on it, and they fail to differentiate it enough from related concepts such as paradigms. As a result, the concept of language games, as used by Astley and Zammuto, has little to do with the work of Wittgenstein.

Certain concepts - such as paradigm, metaphor, and now language game - have gained a certain currency in organization science over the past decade. These concepts have value;

they provide us with useful insights into the phenomenon of organization and provide a mirror in which we can see ourselves in a certain way. But while the field has come to know and use these terms, we have failed to understand the complexity and full ramifications of their application: we have come to know very weak versions of these concepts. This paper has been an attempt to argue for a stronger form of one term - "language game" - and for a greater respect for its original source. Perhaps it is inevitable, given the volume of literature, that certain authors should carry with them a measure of legitimacy in a field where very few have any familiarity with the original work. While there is no requirement that we remain true to the original, it does, however, seem prudent to begin with a more thorough consideration of what has already been done with a particular concept.

Lyotard (1984) has suggested that society in general can be understood as a fabric of language games. That each individual is born into a pre-existing set of games, is shaped by those games, and in turn shapes those games for those who follow. Each person is a node in a complex network of games that constitute the social fabric. The organizational science research community is one small part of the broader social fabric. However, the fabric of language games that makes up the organizational science research community is still vastly complex and it connects with other kinds of games at a myriad of points. Therefore, the legitimation of the research community does not rely on any one argument but varies from game to game - all games, contrary to Beyer's argument, need not be legitimated by managers. Some games are played purely for the benefit of the community itself and these games are completely legitimated by the unspoken contract explicit in the players' participation. Peer review journals, for example, and their importance in legitimating research, are tacit evidence of this legitimation. Other games (teaching, consulting, non-academic publishing) are legitimated in other ways and it is only because Astley and Zammuto posit organizational science as a single language game that the question of

legitimation on such a large scale can arise. But it is not at this level that legitimation occurs, and to discuss as if it did is to be led astray by the way they pose the question.

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Chapter Two
THE SUBJECT OF ORGANIZATIONAL BEHAVIOR

Michael K. Mauws

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Just as the body is formed initially in the mother's womb (body), a person's consciousness awakens wrapped in another's consciousness. Only later does one begin to be subsumed by neutral words and categories, that is, one is defined as a person irrespective of I and other.
- Mikhail Bakhtin, "The problem of speech genres."

In the early part of this century a shift took place in philosophy that is now known as the "linguistic turn". In essence, the effect of the linguistic turn was that it shifted philosophical discussion from talk of *experience* as a medium of representation to talk of *language* as a medium of representation (Rorty, 1992: 373). This shift, in turn, had a major impact upon the philosophy of science. Specifically, it brought about the shift from empiricism to logical positivism that would make possible the hypothetico-deductive account of scientific inquiry (e.g., Hempel, 1966). And when we consider the prevalence of this mode of accounting for scientific inquiry, it is clear that the linguistic turn has had a significant impact within the social sciences.

Indeed, for according to Harré and Gillett (1994) it is the hypothetico-deductive account of scientific inquiry that is, in large part, responsible for the so-called "cognitive revolution" in psychology. By making a space for the positing of hitherto unobserved entities, this mode of scientific inquiry allowed psychologists to move beyond the simple stimulus-response models that had previously been their stock in trade. Freed from the constraints of observable phenomena, psychologists began to posit all sorts of information processing mechanisms with which to account for human behavior. And compared with their earlier research, this truly was a revolution.

More recently, though, philosophy seems to have undergone another shift, which in this case might be called the "discursive turn". In brief, the shift this time seems to be from language as a medium of representation to language as a string of marks and noises used in social practices (e.g., Davidson, 1984). And in this sense, it is a move away from language as some sort of "mirror" of reality (see Rorty, 1979), and a move toward language as simply some sounds and markings we emit in order to make our way in the

world. Or stated slightly differently, it is a move away from language as a *system* of representation and a move toward *language-in-use* or, as it is commonly referred to, discourse. So it is for this reason that I refer to it as the discursive turn.

The effects of this discursive turn are being felt throughout the social sciences and humanities. This includes literary studies (e.g., Derrida, 1978; Norris, 1991), history (e.g., Foucault, 1965; 1970; 1972), feminist theory (e.g., Calás & Smircich, 1991; Weedon, 1987), education (e.g., Belenky et al., 1986; Lave & Wenger, 1991), anthropology (e.g., Bourdieu, 1977; Ortner, 1984), social psychology (e.g., Bruner, 1990; Gergen, 1991) sociology (e.g., Agger, 1991; Bourdieu, 1990) and, more recently, the management sciences (e.g., Mauws & Phillips, 1995; Van Maanen, 1995). But perhaps most importantly for the discussion at hand, the discursive turn is having a significant effect within the discipline of psychology (e.g., Harré & Gillett, 1994; Potter & Edwards, 1992).

In this regard, Harré and Gillett (1994) suggest that the discursive turn is bringing about a *second* cognitive revolution within psychology. And the significance this holds for OB is to be found in the influence that the former has historically had upon the latter (Ilgen & Klein, 1988; Meindl, Stubbart & Porac, 1994; Mowday & Sutton, 1993). In fact, our ways of accounting for organizational behavior have become so similar to those of cognitive psychology that some of our colleagues have started to question whether we may have lost sight of whatever it was that once set us apart from psychology (e.g., Cappelli & Sherer, 1991).¹ Regardless, the point to be made here is that developments in our ways of talking about our 'subject' tend to mirror those in psychology. And for this reason, I would like to suggest that the discursive perspective taking hold therein (e.g., Bruner, 1990; Gergen, 1991; Harré & Gillett, 1994; Potter & Edwards, 1992) may provide us with our best indication of where our field of OB might be headed in the future.

¹ This point is nicely captured in the title of a widely circulated working paper by Robert House and Denise Rousseau entitled, "If it ain't meso, it ain't OB."

So my primary aim herein is to explore this so-called "discursive turn" and its implications for both the 'subject' of organizational behavior and the subject of 'organizational behavior', a distinction whose significance I hope will soon become apparent. And toward this end, what follows is organized into four parts. In the first, I briefly review our current ways of talking about our 'subject' and argue that these ways of talking are, to a large degree, the result of the linguistic turn in philosophy. From this point on, though, the discussion is likely to be relatively unfamiliar to most researchers of OB. For in the second section, I try to show the steps leading up to the discursive turn in philosophy and how this turn, once made, revolutionizes the meaning of what we think and say. So given that we as researchers of OB are not particularly prone to philosophizing – at least not publicly – I hope that we are prepared to try on some new lenses with which to look at our research, which is what I do in the third section. Specifically, I carry through the implications of the discursive turn to our field of OB and describe how it changes our understandings of our research and of our 'selves'. Finally, in the last section of this paper I introduce some of the writings of Pierre Bourdieu and, in particular, his construct of *habitus*. For in my view, Bourdieu's work is perhaps the most convincing among those who are attempting to explicitly accommodate the effects of the discursive turn. In addition, his constructs readily lend themselves to incorporation within OB research. But as I have already said, I begin this paper with a consideration of philosophy's role in our carrying out our research in the manner we currently do.

THE 'SELF' IMAGE OF COGNITIVE PSYCHOLOGY

The accounts of behavior provided by the early psychologists reflected their belief that what they were engaged in was science. By this I mean that they structured their inquiries in relation to their understandings of the scientific method. So for the early pioneers of this so-called behavioral psychology, this meant working within the confines of a crude

sort of empiricism (Harré & Gillett, 1994). It meant limiting oneself to those phenomena that were somehow observable or measurable and, in practice, this translated into stimuli and their apparent responses. For this reason, this approach has come to be referred to in terms of the stimulus-response (S-R) heuristic that guided it.

It was in response to the mechanistic and unthinking portrayal of human behavior associated with the S-R model that the 'cognitive perspective' emerged (Bruner, 1990; Ilgen & Klein, 1988). And in the simplest of terms, what differentiated this perspective from its predecessor was the positing of some role for the physical organism (O) in mediating between stimulus and response. Thus, rather than an S-R heuristic, the cognitive perspective adopted the S-O-R heuristic. And more recently, this has been supplanted by an O-S-O-R heuristic in recognition of the organism's involvement in determining the actual stimulus perceived (Markus & Zajonc, 1985). But regardless of the particular heuristic adopted, what defines cognitive views is "the assumption that people think and that their thoughts play a major role in human behavior" (Ilgen & Klein, 1988: 329).

In this respect, it is important to recognize that what made this so-called "revolution" possible was not the discovery of 'thinking' but, rather, a change in psychologists' conceptions of science (Harré & Gillett, 1994). For it is probably safe to say that the early behavioral psychologists were well-aware of the fact that they and their research subjects were 'thinking'. So unless we want to suggest otherwise, the omission of 'thought' from their theories and experiments must be attributed to something other than "blindness" or ignorance. And in this regard, a likely explanation for why 'thinking' was set aside in this early research is to be found in the model of scientific inquiry with which these researchers were working. Should we accept this explanation, then we can see that it is not that the harbingers of the cognitive revolution somehow "discovered" thinking but, rather, that

they "discovered" a conception of science allowing them to incorporate thinking, or at least some version of it, within their theories and experiments.

The conception of science that allowed these so-called cognitive psychologists to do this originated in the linguistic turn. For as I alluded to above, the linguistic turn had brought about a major shift within the philosophy of science. In the simplest of terms, this shift had to do with the realization that what was important was not that an entity be observable but, rather, that it be *deducible* from that which was observable (e.g., Wittgenstein, 1990 [1922]). In this view, scientists were free to posit any sort of hypothetical entity, so long as there were not empirical observations making possible its logical refutation. In essence, this was because the criterion of admissibility was *falsificationism* rather than the *verificationism* that had previously been advocated (Popper, 1968 [1934]). And this meant that scientists were no longer limited to simple correlations among the observable phenomena of the world. Instead, they were now free to theorize a multiplicity of competing and overlapping entities with which to account for the world. Thus, it was through the introduction of this hypothetico-deductive account of scientific inquiry made possible by the linguistic turn that psychology finally broke out of the epistemological straight jacket of behaviorism.

Perhaps as a reflection of the excitement surrounding computer technology at this time (Kilduff, 1993; Kotre, 1995: 15), the image of the human organism that emerged within these new parameters was that of an "information processor". As Bruner (1990) points out, despite the original aspirations to explore the construction of 'meaning' within the human mind, what began to happen almost immediately within cognitive psychology was an exploration of how human beings *processed* 'information'. What had happened was that researchers had begun positing all sorts of information processing mechanisms in the mind, even concluding at one point that the true test of a theory was its ability to be

programmed into a computer (Turing, 1950). So it was that the mind came to be spoken of as though a set of preprogrammed routines converting inputs into outputs.

This is, then, the way of talking about human behavior that we have inherited in OB (Kilduff, 1993). For example, in describing what they claim is the most influential cognitive orientation within OB – i.e., social cognition – Ilgen and Klein (1988: 329) state that:

the models develop representations of the inputs, processing, and outputs of a cognitive system where inputs are stimuli received by some sensor, process involves the coding, storage, and recall of information, and outputs are the behaviors, attitudes, and/or beliefs that result from the process and may be recycled back into the system.

Thus, just as with a computer, the basic problem solving algorithm is to parse the problem into inputs, processes, and outputs and then construct the necessary processing mechanism to convert the inputs into the desired outputs. And within OB, this is precisely what we do (e.g., March & Simon, 1958).

What we do in OB is describe human behavior in terms of the processing of perceptual stimulants by cognitive programs (e.g., Naylor & Pritchard, 1980). We describe the 'self' as a set of unobservable, hierarchically clustered cognitive structures (Ilgen & Klein, 1988). We speak of scripts and schemas (e.g., Gioia & Poole, 1984; Lord & Foti, 1986) as though we were speaking of programming languages (perhaps even more so since the advent of object-oriented languages such as C++). And finally, we speak of 'controlled' and 'automatic' processing (e.g., Ashforth & Fried, 1988) as though wrestling with the problem of "real time" computing. Thus, in an ironic twist of fate, we appear to be garnering insights into cognition from a technology that is, in large part, supposed to be modeled on it.

But where we run into difficulty is when this analogy fails to hold. For we know that the wonder of computers is their ability to repetitively perform the same set of operations without variation. Unfortunately, we also know that this is anything but the case when it

comes to human behavior (e.g., Potter & Wetherell, 1987). Thus, the result has been that we have tried to posit a variety of mechanisms that might account for the less than perfect functioning of the human information processor. We speak of conditions such as 'bounded rationality' (e.g., Simon, 1976; cf. Mumby & Putnam, 1992) to account for its limited capacity; we speak of 'framing' (e.g., Kahneman, Slovic & Tversky, 1982) and the 'mindlessness of organizational behavior' (e.g., Ashforth & Fried, 1988) in order to suggest that the problem lies with a less than complete set of "inputs". And more recently, we have begun to explore 'meso' research (e.g., Mowday & Sutton, 1993) on the assumption that the difficulty has been in our not considering "inputs" from other levels of analysis. So although we are managing to preserve the image of the information processor, the "equants" and "epicycles" necessary to do so are calling into question its validity. Thus, it is because our counterparts in psychology had this very same experience not that long ago (see Bruner, 1990; Gergen, 1991) that we may want to explore the 'discursive turn' that seems to have steered some of the latter away from the cognitive perspective (e.g., Harré & Gillett, 1994; Potter & Edwards, 1992).

THE DISCURSIVE TURN

In trying to explain the discursive turn, the first thing I would like to do is introduce a couple of terms that I borrow from the discussions of Ferdinand de Saussure (1983). The first of these, *langue*, I wish to use to refer to language as a system. In speaking of *langue*, I am referring to a language in its entirety. If it is the English language I wish to speak of, for example, *langue* would signify all the available words and the rules (grammar) governing their combination. Or to put this somewhat differently, *langue* is what I have at my disposal each time I go to speak. And *parole* is, in a sense, the complement of this. *Parole* refers to the actual act of speaking; it is our use of language in situated contexts. So to tie the two together, it is from *parole* that *langue* is inferred but, nevertheless, it is *langue*

that seems to make *parole* possible. Or as Saussure (1983) describes it, *langue* is language examined synchronically, while *parole* is language examined diachronically.

The discursive turn is, in these terms, a shift in focus from *langue* to *parole*, it is a shift from language as system to language-in-use. And ironically, the person most often credited for this shift is Ludwig von Wittgenstein (1889 - 1951). What is ironic about this is, first of all, that it is Wittgenstein who is also given credit for the linguistic turn (Bergmann, 1992 [1953]). But more importantly, what makes this ironic is that the views espoused in his *Philosophical Investigations* are, to a large degree, diametrically opposed to the earlier views expressed in *Tractatus Logico Philosophicus*. So because Wittgenstein is readily identifiable with both *langue* and *parole*, following his transition is perhaps our best strategy for comprehending what the discursive turn is all about. For this reason, it is his writings that I will focus upon here in order to elucidate what, exactly, I am referring to in speaking of the 'discursive turn'.

One Man's Search for 'Meaning'

Despite the shift in his views between *Tractatus Logico Philosophicus* and *Philosophical Investigations*, it was constancy rather than fickleness for which Wittgenstein was known (Monk, 1990). And in this regard, there are two things we should know about him. The first is that, throughout his life, what interested Wittgenstein was the nature of language and its relation to the world (Hanfling, 1989; Kenny, 1973). Or to put this in slightly different terms, we might say he was interested in the notion of 'meaning'. And this, in turn, ties into the second thing we should know about Wittgenstein which is that, throughout his intellectual life, he was adamant about the distinction between *saying* and *showing* (Monk, 1990). In his view, to *say* that a word corresponds to something in the world or, alternatively, to *say* what a word means, in no way *shows* that this is the case.

Thus, it was his striving to *show* that which is the case rather than *saying* it that, to a large degree, would lead him to the "breakthroughs" for which he is now known.

The first of these breakthroughs came in the form of *Tractatus Logico Philosophicus* and was, for the reasons just described, Wittgenstein's attempt to *show* how language corresponds to the world. For the starting point of his investigation was with the assumption that the meaning of a proposition is the fact to which it corresponds. And for this reason, the challenge as he saw it was in *showing* how it is that a proposition might correspond to the fact from which it derives its meaning. The problem, in other words, was that of representation.

Wittgenstein's solution to this problem was the picture theory of meaning which, in the simplest of terms, amounted to the belief that propositions "picture" the world. This followed from his view that propositions are the material traces of thoughts, and that thoughts, in turn, are pictures of the facts that are the world. Thus, a proposition means what it does because it presents a picture of the fact to which it corresponds (see Kenny, 1973). When we say that "the fork is to the left of the knife", for example, we know what is meant because the word "fork" is literally to the left of the word "knife" in this sentence. Nevertheless, much of our speech involves complex propositions that mask this correspondence and, for this reason, a large portion of *Tractatus* is devoted to showing how complex propositions can be broken down into 'atomic propositions' that literally do picture the world.

The significance of this picture theory of meaning for the discussion at hand is its implication that truth has to do with a relationship of correspondence between proposition and fact. In other words, true propositions here are those that accurately "picture" the fact to which they correspond. Conversely, propositions that fail to "picture" some fact in the world are, in this view, considered to be false. Either way, because it is the world rather

than human beings that is determining the truth-value of propositions here, knowledge – the collection of true propositions – is understood to be neutral and objective. So by showing *how* meaning is possible, and specifically through anchoring it in the world, Wittgenstein's *Tractatus* provided the foundation for subsequent perspectives such as that of logical positivism (e.g., Donaldson, 1992).

The "Later" Wittgenstein²

If the *Tractatus* was Wittgenstein's attempt to show *how* meaning was possible, then his subsequent work was his attempt to show that 'meaning' is *not* what we think it is. For what would subsequently lead Wittgenstein to abandon the views espoused in his *Tractatus* was the realization that he had, for the most part, taken the notion of 'meaning' for granted. As I said above, he had assumed that the most pressing problem of philosophy was that of representation– specifically, how it was that propositions were able to represent the facts to which they corresponded. However, what he subsequently came to see is that this is only *the* problem of philosophy if one accepts at the outset that the meaning of a proposition is the fact to which it corresponds. But if, on the other hand, meaning is something other than this, then it is likely that *the* problem of philosophy is something very different. And in this respect, Wittgenstein's later work suggests that *the* problem of philosophy might simply be the meaning of 'meaning'.

In this sense, *Philosophical Investigations* can be seen as Wittgenstein's attempt to *show* us that the meaning of a word or proposition is not to be found in that to which it corresponds. And it is this break with the *nomenclaturist* view of language that represents Wittgenstein's second philosophical "breakthrough". For in probing the meaning of

² *Tractatus Logico Philosophicus* was the only major work published during Wittgenstein's lifetime. Nevertheless, he continued to write after this volume was published and, thus, it is through the posthumous publication of these latter writings that we have come to know his later views. In this respect, it is *Philosophical Investigations* that is generally recognized as indicative of Wittgenstein's later views.

'meaning', what Wittgenstein subsequently concluded was that it is not through that which they correspond to that our words acquire their meaning but, rather, in how we use them in particular social contexts. In this later work, he suggests that the meaning of language is to be found in what it accomplishes in the actual situation in which it is used—that apart from its use in such contexts, it has no meaning. What he had concluded, in other words, is that it is in *parole* rather than *langue* that meaning resides.

So in *Philosophical Investigations* Wittgenstein tries to show us that the nomenclaturist conception of language is flawed. And toward this end, he opens with a passage from Augustine's *Confessions* in which the latter describes how it is that he learned to speak. Much like the account of language given in *Genesis*,³ Augustine describes a process of ostensive definition in which objects were repeatedly pointed to and their names enunciated for him to hear. In this manner, or so he says, he learned to speak these words and to use them to express his own desires.

Taking this as his starting point, Wittgenstein sets out to show that, in fact, very few of our linguistic capabilities could ever be acquired in this manner. And in this regard, his strategy is to merely give example after example of words whose usage we could not have learned this way. For instance, in a particularly salient example he asks,

[a]re "there" and "this" also taught ostensively?—Imagine how one might perhaps teach their use. One will point to places and things— but in this case the pointing occurs in the use of the words too and not merely in learning the use. (1958: §9)

Thus, in pointing to the fact that 'pointing' is, in this case, part of the proper use of the words in question, what Wittgenstein is trying to show us is that relatively few of our words can be defined by pointing to some fact to which they correspond.

³ "And out of the ground the Lord God formed every beast of the field, and every fowl of the air; and brought them unto Adam to see what he would call them: and whatsoever Adam called every living creature, that was the name thereof" (quoted in Harris, 1988: 7).

But if Wittgenstein's point were merely that *some* of our words cannot be defined ostensively, whatever else he might have to say could probably have been incorporated within existing frameworks. Thus, what set his work apart and brought about the breakthrough with which it is associated is his conviction that *none* of our words are defined in this manner. More specifically – since the latter point depends on our definition of “define” – the point Wittgenstein was trying to make in examples such as that given above is that we do not learn the ‘meanings’ of words, at least not in the traditional sense of this term. Instead, what we learn is how to *use* words in particular ways in particular situations in order to accomplish particular ends. So in this sense, he was not suggesting that we learn ‘meanings’ through means other than ostensive definition. Rather, he was suggesting that ‘meanings’ *per se* do not exist. Instead, they are created through our use of language in particular contexts. Thus, it is because this represented a *total* abandonment of the conventional sense of ‘meanings’ that Wittgenstein's *Philosophical Investigations* represented a complete break with the approaches that had preceded it.

In keeping with his emphasis upon *showing* as opposed to *saying*, Wittgenstein tried to show us that the source of much of our confusion is to be found in our use of the word “meaning”. For the difficulty, as he saw it, is that when we receive a response to our query, “What is the meaning of X?”, we believe that what others are actually providing us with is the ‘meaning’ of X when, in fact, what they are actually *showing* us is our agreed upon way of responding to the manner in which we have used the word “meaning”. As he says,

“The meaning of a word is what is explained by the explanation of the meaning.” I.e.: if you want to understand the use of the word “meaning”, look for what are called “explanations of meaning”. (Wittgenstein, 1958: §560)

What Wittgenstein wanted us to see, thus, is that the ‘meaning’ of “meaning” is not simply “that to which a word corresponds.” Instead, he wanted us to realize that

[f]or a *large* class of cases – though not for all – in which we employ the word "meaning" it can be defined thus: the meaning of a word is its use in the language.
(1958: §43)

For in this sense, 'meanings' are not the putative facts to which words correspond but, rather, the uses to which the words can be put. And if this is the case, then it is not rules of representation that determine meanings but, rather, our rules of use. For this reason, Wittgenstein was adamant that it is *parole* rather than *langue* in which we should be interested.

Language games

In exploring our use of language – *parole* – Wittgenstein often spoke of what he called "language-games". In doing so, he wanted to "bring into prominence the fact that the *speaking* of language is part of an activity, or of a form of life" (Wittgenstein, 1958: §23). And in saying this, what he was trying to draw our attention to is the fact that our using language is much like making moves in a game. The words, in this sense, are merely the pieces being moved around the game-board in order to accomplish our desired ends. And just as the same piece can be used in different ways within different games, Wittgenstein wanted us to see that our use of words, and hence their meanings, depends on the game in which we are participating. And he wanted us to see that the corollary of this is that the words, when not being used, have no inherent meanings. In much the same manner that game pieces are merely bits of wood and plastic in the absence of a game, words, in his view, are merely "marks and noises used by human beings in the development and pursuit of social practices" (Rorty, 1992: 373). So in this sense, 'meanings' are not revealed by getting "inside" words to unearth their referents. Rather, 'meanings' are revealed by getting a sense of the social practices in which they can be used.

So in speaking of language-games and, in particular, in speaking of language-games in the plural, Wittgenstein was trying to draw our attention to the fact that we participate in

a multiplicity of language-games as we go about our day-to-day activities. The telling of a riddle, speculating about an event, forming and testing hypotheses: each of these is an example of a language game with its own set of rules (Wittgenstein, 1958: §23). And within each of these games, our words can accomplish something very different, or nothing at all, depending upon the set of rules associated with that game. Nevertheless, if we are unsatisfied with how words are being used within a game, we can, in effect, play a "meta-game" in which we negotiate new rules and revise old ones for the game in which we are interested. For it is in this manner that the 'meanings' of words – what we can accomplish in using them – is determined. In other words, it is our agreement in usage rather than any correspondence with the world that decides the meanings our words can assume.

The significance of this shift for the discussion at hand is that it effectively undermines the possibility of truth as neutral and objective in the sense associated with *Tractatus*. For with the latter, truth was objective because our use of language was, in effect, governed by reality. Because the meaning of a proposition was the fact to which it corresponded, it was the world that was "deciding" what was true or false. However, with *Philosophical Investigations*, what Wittgenstein was now suggesting is that it is not facts that determine the truth or falsity of our statements but, rather, rules of use. And what this implies is that true statements are those that reflect agreed upon ways of speaking rather than those that correspond to some fact in the world. Or expressed somewhat differently, it is statements *sui generis* rather than states of the world that are true or false in this view. For as Rorty says,

Truth cannot be out there - cannot exist independently of the human mind - because sentences cannot so exist, or be out there. The world is out there, but descriptions of the world are not. Only descriptions of the world can be true or false. The world on its own - unaided by the describing activities of human beings - cannot. (Rorty, 1989: 5)

As a result, it is our agreement in how language is to be used that makes it possible to declare statements true or false. When we declare a statement true or false, in this view,

what we are really doing is agreeing or disagreeing with the manner in which language has been *used*. What we are *not* doing is agreeing upon the particular state of affairs being suggested.

To elaborate, it is for this reason that I suggested above that the discursive turn represents a shift from language as a medium of representation to language as marks and noises that we emit to get by in the world. For when language is no longer understood to correspond to the things of the world, then there is no basis for assuming that our use of language is the means whereby one individual conveys a particular state of affairs to another (see Boje, 1991). All we can say, in contrast, is that we emit these marks and noises and, if the person to whom we are writing or speaking is familiar with the ways in which we have used them, then they are likely to respond in the manner we had anticipated. Nevertheless, what is *not* going on here, at least from the later Wittgenstein's perspective, is the transposition of some image from one speaker's mind to that of another. For it is not because of what these words *mean* but, rather, because of the agreed upon rules of *use* that the person to whom we speak responds as they do. So it is that Wittgenstein (1958: §43) says that "the meaning of a word is its use in the language."

Language-games were, in this sense, Wittgenstein's heuristic for determining 'meanings', but only in the particular sense that he was using the latter term. For as was already stated, in making the shift from *langue* to *parole* he had, in effect, re-located 'meanings' from facts-of-the-world to rules-of-use. And in doing so, what he was trying to do was make a space for the possibility of polysemy. For as he said, when we look at a word

[i]t is like looking into the cabin of a locomotive. We see handles all looking more or less alike. (Naturally, since they are all supposed to be handled.) But one is the handle of a crank which can be moved continuously (it regulates the opening of a valve); another is the handle of a switch, which has only two effective positions, it is either off or on; a third is the handle of a brake-lever, the harder one pulls on it, the harder it brakes; a fourth, the handle of a pump: it has an effect only so long as it is moved to and fro. (Wittgenstein, 1958: §12)

And so, in speaking of language-games he wanted us to see that just as the 'meanings' of words are not determinable in relation to some fact in the world, neither are they determinable in relation to a single language-game in which they are used, for this would merely be another form of correspondence (e.g., Saussure, 1983). Instead, he wanted us to see that words acquire their meaning within particular language-games but, nevertheless, they can be deployed within a multiplicity of language-games. For this reason, it is language-games, understood as being collections of social practices, rather than individual words or propositions in which we should be interested. It is these games, in other words, rather than any correspondence with the world, that determine whether our use of language is right or wrong and, thus, whether our statements are true or false.

So to briefly sum up, the discursive turn is associated with the view that our use of language amounts to a collections of sounds and markings that we use to accomplish our desired ends in the world. And in this sense, if we speak of language as representing the world, it is *not* because this is the case but, rather, because this is our agreed upon way of doing so— it is a language-game in which it is acceptable to do so. Nevertheless, the meaning of a particular word, at least in this view, can only be determined in relation to its use in a particular context and, as described here, that context is best described in terms of a language-game. For the latter draws our attention to the fact that the rules that determine how our words can be used and, in this respect, what our words mean is, in practice, the outcome of human negotiation. And what this implies is that, in agreeing upon what is true or false, what we are actually doing is agreeing upon the particular sounds and markings we will use and the particular manner in which we will use them. But most importantly, what this implies is that this is not agreement in what *is* or what *was* but, rather, on what *will be* in the future.

CHANGING THE 'SUBJECT'

The discursive turn has two important effects on how we talk about our research and, as a result, on how we carry out our research. First of all, for reasons that I will discuss below, it leads us to speak of whomever or whatever it is that is responsible for organizational behavior in very different terms. For most, this is expressed in the use of constructs such as 'subjectivities' or 'sites of subjectivity' (e.g., Knights, 1991; 1992; Linstead & Grafton-Small, 1992; Townley, 1993; Weedon, 1987). And by speaking in this manner, the intention is to draw attention to the 'positioning' of the 'subject' when accounting for human behavior (Bourdieu, 1990; Foucault, 1980; 1982; Harré & Gillett, 1994). Furthermore, by speaking of our 'subjects' in these terms we acknowledge that they are not what we once thought they were – i.e., information processors. And this, in turn, brings us to the second, and perhaps the more noteworthy effect of the discursive turn, which is that it forces us to acknowledge the importance of *understanding* our 'subjects' as opposed to accounting for or explaining them – what Weber called *verstehen* (Winch, 1958)

Lest this be unclear, recall that the cognitive perspective encourages us to speak as though our constructs represent what is *actually* going on in the mind. Although we might not be able to come into direct contact with the entities hypothesized, the belief is, nevertheless, that these entities do exist (at least until proof exists to the contrary). In contrast, because the discursive perspective foregoes any notion of correspondence between propositions and the facts of the world, it encourages us to see our words and assertions for what they are; i.e., marks and sounds we emit to make our way in the world. As a result, for researchers who make the discursive turn, theories no longer represent descriptions of the world but, rather, ways of speaking that can get things done in the world (Rorty, 1989; Weick, 1992). By speaking in the terms of a particular theory, for example, we might influence the way our colleagues conduct their research and the sorts of statements they

make to their students. Nevertheless, should we be successful, this is not an indication that we have somehow got things "right". Instead, it is an indication that we have – perhaps through our use of other sounds and markings – managed to convince others of all that can be accomplished by speaking in the manner we have been suggesting.

The 'self' image of researchers

I suggested above that our usual way of speaking is in terms of information processing mechanisms along with the inputs and outputs associated with them. The significance of this is that, by speaking in this manner, what we do is portray the mind as a space in which the processing of inner representations takes place. We imply that the mind perceives a world around it in the form of representations of that world, and that human behavior is the outcome of how the mind processes these explicit representations. But perhaps even more importantly, by speaking in this manner, and in conducting experiments in these terms, what happens is that we come to believe that this is how the mind *actually* works. When we say, for example, that work performance is mediated by attitudes, we seem to be suggesting that *all* individuals actually have attitudes, that what we are calling attitudes have *always* existed, and that these very same attitudes will *always* be a part of human cognition. In other words, we treat attitudes as though things-in-themselves.

But when we make the discursive turn, we acknowledge that we have no way of *showing* that such entities might exist. Although we might speak of them and account for behavior in terms of them, there is no point at which we might unequivocally establish that such an entity resides in the mind. What we find is that each time we *say* what attitudes are or how they operate, what we actually *show* is the various ways in which we have agreed that "attitudes" can be used in our discussions. What we never show, and what Wittgenstein has told us we will *never* be able to show, is that there is some *thing* in the world to which the word "attitude" corresponds. And for the very same reasons, neither

will we be able to show that 'attributions', 'affect', 'motivation' or any other such constructs exist. In each case, what we demonstrate instead are our agreed upon ways of talking.

Nevertheless, as researchers, it is clear that we use these terms in ways that differ from others' uses of them. Not that we are always using these terms in these particular ways, mind you, but when we speak *qua* researchers, we are not using these terms in the same manner that we would with, say, friends or family. With the latter, for instance, it is unlikely that we would ever ask them to explain what they "mean" when they speak of 'attitudes'. Nevertheless, in an academic context this is, if not a common question, a question for which we would expect the researcher of 'attitudes' to be prepared. In fact, it is familiarity with this practice of defining one's constructs, as well as being familiar with a variety of other related practices, that results in someone being perceived as a 'researcher'. And in this sense, we might even go so far as to say that it is the particular set of practices that we participate in that constitutes us as participants in the field of organizational behavior.

This is, in many respects, the point that Astley and Zammuto (1992) were trying to make in suggesting that 'management practice' and 'organization science' are two different language-games. Nevertheless, in my opinion, this is to stretch the concept of language-games considerably further than the micro-activities Wittgenstein (1958) seems to have intended it to describe (Mauws & Phillips, 1995). For this reason, it would seem to be more apt to refer to our field as simply a 'community of practices' (Lave & Wenger, 1991). For what this does is draw our attention to the fact that what defines our community is the particular repertoire of practices – discursive *as well as* non-discursive practices – that we have agreed to use in our interactions with one another, and *not* the particular individuals associated with it. So when my colleague at the next desk discusses household chores with me, at that moment we are not interacting as members of our

research community; we are instead interacting as members of a geographically defined community, perhaps one crudely demarcated by the four walls around us. Thus, even if we go on to discuss our respective 'attitudes' toward housework, we have yet to "join" the research community. But that said, when we begin to talk about operationalizing people's attitudes with respect to housework, and start conducting experiments to test those attitudes, then at that point we have more than likely "joined" the research community. And what I would hope this illustrates is that 'organization science' should be thought of as neither a language-game nor a community of researchers; i.e., because it is not the researchers themselves but rather the practices they engage in that constitute it as a community, it is better thought of as a community of practices.

Speaking in terms of a "community of practices" also helps us to appreciate Wittgenstein's point that our ways of talking constitute "forms of life". As he expresses it,

"So you are saying that human agreement decides what is true and false?" - It is what human beings say that is true and false; and they agree in the language they use. That is not agreement in opinions but in form of life (Wittgenstein, 1958: §241).

And so, when we as researchers agree to speak of cognition in terms of 'attitudes' and 'emotions', what we are *not* agreeing upon is that these entities actually exist in the mind. Instead, in light of the discursive turn, what we are actually agreeing upon is the shape of that distinctive form of life known as "cognitive research" and the identities of the so-called "cognitive researchers" associated with it. In other words, we are agreeing upon the particular set of marks and traces that we will use as researchers and the particular ways they will be used. We are, for example, agreeing that it will be acceptable to say, "How did you operationalize 'attitudes' in your experiment?" And we are agreeing that an acceptable response will be to cite some statistics, or to speak of some questions on a survey. Nevertheless, this is not to agree on what we *mean* by 'attitudes' or to agree that 'attitudes' *exist*. Once again, it is simply to agree on how the term "attitudes" is to be used within the community of practices that is known as the field of organizational behavior.

But when we see ourselves as a community of practices rather than a community of individuals possessing "knowledge" of the entities of which the mind might be comprised, we begin to see our research in a very different light. For no longer can we speak as though the 'objects' of our discourse actually reside in the mind – as we have seen, Wittgenstein robbed us of this possibility. As a result, neither can we speak as though it is these entities in themselves that we are exploring through our research. Instead, the conclusion we are led to is that, through our research, what we are actually exploring is new and hopefully more effective ways of talking to one another *qua* researchers. That said, if these ways of speaking can be *shown* to be more effective in accomplishing particular outcomes in which others are interested, then it is likely that other members of our research community will agree that this is the manner of speaking we should adopt. And in this manner, this may, at least for a time, become the way in which one makes "true" statements within our community.

What we see, thus, is that in this view our constructs are no longer understood to be "deeper" in the mind than are the accounts of behavior provided by the individuals themselves. Rather, ours are merely the agreed upon ways of talking within a particular community. But as such, they become our ways of thinking and, hence, our ways of carrying out research. Because they are our agreed upon ways of talking, it is in terms of them that we construct our investigations and write up our results. For this reason, what we find is that these agreed upon ways of speaking within this community of which we are a part can have a significant impact on the sorts of behavior we engage in as members of the community. So although they might not correspond to the things of the world, in the sense just described we can say that the terms through which we talk about the world do, nevertheless, have an impact upon the sort of world we find ourselves inhabiting.

What is perhaps most important here is that, in acknowledging this, we are also acknowledging that we are unable to say that *our* constructs represent the 'true' workings

of the mind. And in doing so, we begin to see that our entire approach to our 'subject' may be desperately in need of an overhaul. For unless we wish to suggest that "we" are somehow different from the individuals we study, we can only conclude that the behavior of these "other" individuals is, similarly, guided by the community of practices of which they are a part, or in which they participate. And it is for exactly this reason that we must *understand* the 'subjects' of our research if we wish to predict their behavior. In other words, behavior is not to be understood through an ever-more detailed explication of the 'individual'. Instead, as I discuss at greater length in the next section, behavior is to be understood through the positionings of the subject that result from individuals being situated within particular communities of practices.

Nevertheless, what is perhaps unique about the ways of speaking we have as researchers is that they are specifically oriented toward subsuming the accounts and behaviors of others. More succinctly, they are negotiated with generalizability in mind. For having explored a greater diversity of examples through our investigative efforts, we are often able to propose ways of speaking capable of expressing a greater variety of experiences than if we had limited our sample to only our own experiences. Thus, it is precisely this that enables us to engage in our trademark social practice of providing generalizations and probabilities. However, in doing so, what we now find is that we are in no way describing how things *actually* are or how they *actually* came to be. What we are saying, instead, is that ours seems to be a quite powerful way of talking about this phenomenon in the aggregate. As a result, if anyone is trying to anticipate the outcomes associated with this phenomenon, or if anyone is trying to facilitate or impede the occurrence of this phenomenon, we are merely inviting them to try speaking about it in the manner that we have developed.

So to briefly sum up this first point, the discursive turn leads us away from the idea that our ways of speaking represent entities located "deeper" in the minds of individuals

than are their own accounts of their behavior. Instead, it leads us toward the idea that our ways of speaking are simply that: ways of speaking (albeit oftentimes quite convincing ways of speaking). It moves us away from the idea that what we are doing is explaining the behavior of others and, instead, forces us to recognize that what we are really doing through our research is finding new ways of talking about behavior, perhaps with the aim of changing behavior within *our* community of practices. Nevertheless, if and when these ways of speaking are adopted within other communities of practices, then at that time they will affect behavior outside our community. But given how seldom it is that our ways of speaking never "trickle down" to these other communities (e.g., Porter & McKibbin, 1988), we must see our research as being, first and foremost, about finding new ways of talking and, perhaps, new ways of influencing behavior, within the community of practices known as the "field of organizational behavior" (Mauws & Phillips, 1995).

Talking to/about our 'selves'

Because the discursive turn encourages us to resist speaking of language as though a means of representation, it also encourages us to adopt a fundamentally different perspective on our 'subject', by which I mean the agent of organizational behavior. And this is the second major effect of the discursive turn. For whether it be Subjectivists or Objectivists (e.g., Burrell & Morgan, 1979), the assumption has always been that it is in terms of individual human beings that we must account for human behavior. For Subjectivists, this involves getting to know the worlds inside the minds of those who act (e.g., Prasad, 1993). And for the latter, as is exemplified by the cognitive perspective, this involves getting to know the universal cognitive machinery contained within each and every human being (e.g., Meindl et al., 1994). Either way, the approach is to account for behavior in terms of the individual with which it is associated. In brief, it is to see the individual and the 'subject' that acts as being one and the same entity.

It is this identity that the discursive perspective seeks to blur (Dews, 1987; Henriques et al., 1984; Weedon, 1987). For as I said above, for those who adopt this perspective it is common to speak in terms of 'subjectivities' and 'sites of subjectivity'. In doing so, the intention is to draw attention to the fact that a human being is not identical with the 'subject' that acts; instead, it is intended to draw attention to the fact that any one human being is capable of occupying numerous 'sites of subjectivity' as they go about their day to day activities. Or to pick up on an earlier discussion, individuals situate themselves within numerous communities of practices in going about their daily affairs. And each of the 'sites of subjectivity' they occupy therein carries with it its own particularities that may or may not cohere with those of the other 'subjectivities' with which they are associated (Gergen, 1991).

So in this view, the individual is more than capable of contradictory behavior (e.g., Potter & Wetherell, 1984). For what determines the behavior engaged in is, to a large degree, the particular community of practices in which an individual is immersed at the time they act— the particular 'site of subjectivity' they are occupying at that moment in time. So in this sense, the individual and the 'subject' are very different entities. In brief, when we abandon the correspondence conception of truth we do not just abandon the possibility that our words might correspond to the facts of the external world. In addition, we also abandon the possibility of our words corresponding to any images, states or processes "within" us (Wittgenstein, 1958). We are left without any way of showing that our talk of 'desires', 'beliefs', 'attitudes', etc., convey some inner states of being. And perhaps most troublesomely, we are left without any way of showing that there is some thing within us that might be referred to as our 'self'. Instead, all we are able to show is that these are our agreed upon ways of talking. And like all our other words, they are merely more marks and traces to make our way in the world, made meaningful only through their use within particular social practices (Rorty, 1989).

Thus understood, the 'self' is better understood as cultural myth rather than natural fact. Mythical not in the sense of being untrue but, rather, in the sense of providing us with a vehicle for making sense of our world (see Barthes, 1972). As Jermier (1985: 78) notes, "[m]yths are culturally managed modes of explanation that account for social circumstances." And in this sense, the 'self' provides us with a particularly powerful myth with which to account for human action (e.g., Gephart, 1993). But as Jermier (1985: 78) also notes, "[t]he forms of consciousness through which the world is comprehended results from a synthesis of myths in the broader cultural settings and active, personal reflection." Thus, it is not just that we account for the world around us in terms of the 'self'. Over time this myth permeates much deeper effectively constituting our consciousness *in terms of a 'self'*.

What this implies, then, is that our 'selves' are the product of our immersion within particular communities of practices (e.g., Gephart, 1978; 1993; Henriques et al., 1984; Lave & Wenger, 1991). And that through the particular ways of speaking thus acquired, we create the possibility of an internal dialogue in which we create and, thus, get to know our 'selves' (Kotre, 1995; Weedon, 1987). But because our words and ways of speaking are no longer understood to represent something that is somehow "beyond" them, the 'self' is no longer seen as an image or essence within the "theatre" of the mind. Instead, "self" here is merely one more piece to be used in the numerous language-games we play. Talking about our 'self', in this view, is merely one more social practice— one that is as much a product of intersubjective negotiation as any other. It is not that we have a 'self', but that we speak in terms of our "selves" and that by doing so we situate ourselves within a particular nexus of relationships (Bruner, 1990). If the 'selves' of which we speak are differentiable from the 'selves' of others, then this can only be as a result of each of us using this term in the context of a different community of practices. For as I suggested above, it is in this manner that all of our terms acquire their meanings.

So in speaking as though each of us possesses a unique sense of "self", what we are actually suggesting is that within each individual is, so to speak, a unique community of practices reflecting the particular social contexts in which that individual has been immersed (Bhaskar, 1989; Bourdieu, 1977; 1990). For as Harré and Gillett (1994: 22) suggest, using "discourse" here in much the same sense that I have been using "communities of practices",

the study of the mind is a way of understanding the phenomena that arise when different sociocultural discourses are integrated within an identifiable human individual situated in relation to those discourses (Harré & Gillett, 1994: 22)

And similarly, Weedon (1987: 33) notes that

[a]s we acquire language, we learn to give voice - meaning - to our experience and to understand it according to particular ways of thinking [and] particular discourses... These ways of thinking constitute our consciousness, and the positions with which we identify structure our sense of ourselves" (Weedon, 1987: 33).

So in making these statements, these writers are suggesting that the 'self' is not something we are born with. Instead, it is the product of a particular repertoire of social practices inscribed upon us, whether this be in the mind or elsewhere (see below). And as such, it is something quite different from that which is implied by our usual ways of speaking.

It is for this reason, then, that Bruner (1990) speaks of the 'self' as being "dispersed" or, alternatively, as being "populated" and "populating". In doing so, he is trying to draw attention to the fact that our 'self' in the sense of the whole individual is never present in its entirety- that it is, instead, dispersed among the numerous 'sites of subjectivity' it occupies. For this reason it is useful to think of it as "populating" in that each of us populates various communities of practices with our 'self'. And conversely, we can think of our 'self' as being "populated" with the social practices of these communities in which it has been immersed, as well as by the 'subjectivities' negotiated for us and by us within these communities. But if we do so, to the degree that we continue to speak in terms of a 'self', it must be as that which is trying to reconcile the competing demands and

contradictory logics of its many 'subjectivities' (Harré & Gillett, 1994; Henriques et al, 1984). And this, then, is the version of 'self' reflected in Gergen's (1991) speaking of the 'saturated self'. For as he sees it, our ability to travel further and more frequently, and the development of new modes of communication such as the Internet, create a situation in which individuals' 'selves' are being populated with an ever-increasing number of 'subjectivities'. As a result, reconciliation becomes an increasingly daunting task, while "multiphrenic" personalities increasingly become the norm (Iyer, 1993).

But regardless of whether our 'selves' are, in fact, becoming saturated, what is important here is that a growing number of psychologists are attempting to incorporate the insights associated with the discursive turn into their understandings of human behavior. At the very least, they are finding it useful to draw a distinction between the agent of action and the individual human organism. And in this respect, writers such as Gergen (1991) and Bruner (1990) are trying to explore what this means with respect to our sense of our 'selves'. Or stated slightly differently, they are interested in helping us find ways of understanding our 'selves' that are compatible with what is increasingly being referred to as the "postmodern condition" (e.g., Bauman, 1993; Lyotard, 1984). Others, however, such as Harré and Gillett (1994) and Potter and Edwards (1992), are arguing for a psychology that approaches the 'mind' in a fundamentally different way. Harré and Gillett (1994), for example, want to explore the mind as a set of skills as opposed to information processing mechanisms or inner states and processes. By doing so, they are trying to rehabilitate each of psychology's traditional domains to take stock of the insights associated with the discursive turn. And if they and others are successful, it may not be long before the cognitive perspective is as foreign to us as are the S-R models of behavioral psychology. So it is for this reason that I am suggesting that we in OB may want to begin incorporating within our research the sorts of observations these researchers are making.

THE 'SUBJECT' OF ORGANIZATIONAL BEHAVIOR

In the preceding section I tried to draw out two very important implications of the discursive turn. The first of these was that the discursive turn robs us of the belief that what we are doing with our research is exploring the 'true' workings of our 'subjects'. The argument I sought to make was that, no matter how powerful our ways of speaking might appear to be, post-Wittgenstein, there is no way for us to show that these concepts in terms of which we speak correspond to actual mechanisms within the mind. In the end, they are merely the ways of speaking we have negotiated in order to accomplish the particular outcomes in which we are interested.

But in contrast with so-called Subjectivist accounts, I did not conclude from this that we must resign ourselves to gathering individuals' own accounts of their cognitive machinations and inner states of being. For as I went on to argue, neither can individuals show that their descriptions correspond to actual states of being or the actual processes through which particular outcomes have been arrived at. Rather than being that most private of possessions given to us at birth, what I argued instead was that our 'self' is, in fact, the linguistically constructed *product* of our immersion within particular social settings. So despite our 'self-descriptions, despite how well we might feel we know our 'self', to assert that this is what we *are* is, to some degree, to sell ourselves "short". It is, in effect, to reduce our entire being-ness (and our behavior) to the articulable and, thus, to 'consciousness'. However, instead of approaching the 'self' as though this is what we are, the discursive turn encourages us to approach our *use* of the term "self" as a means whereby we weave our being-ness within a particular community of practices. And in this sense, it is not that we *have* a 'self', but that we speak *in terms of* a "self".

In light of these observations, what we require within OB, and within the social sciences more generally, is a way of approaching our 'subject' that avoids equating it with an ever-

present information processor as the cognitive perspective and the 'rational actor' perspectives that it has spawned lead us to do. But at the same time, we require an approach that avoids that "other" trap of reducing our 'subject' to a mindless automaton enacting the scripts and schemas it has unwittingly acquired (e.g., Ashforth & Fried, 1988). For despite the fact that both of these perspectives are well-suited to describing particular types of behaviors, both do so by excluding a great deal of behavior which cannot be described in their respective terms. So for this reason, what is required is a way of speaking that transcends the antinomy between the two while preserving the gains associated with each (Bourdieu, 1990). And in this regard, the most promising perspective that I have encountered revolves around Pierre Bourdieu's (1977; 1985; 1990) construct of *habitus*. So what I would like to suggest and encourage through this final section is that, in the future, OB research may revolve around the exploration of the 'organizational habitus'.

Getting Beyond Language

As Charles Taylor (1993) describes it, the utility of "habitus" lies in its ability to capture a level of social understanding that is necessary to function in the world, but which is, nevertheless, more or less unarticulated. Specifically, he sees it as introducing the body and the 'other' into our notion of understanding. This is important, he says, because our investigations of human behavior have traditionally been premised upon a sort of 'monological consciousness'. By this he means that we have conceived of the 'subject' as though existing apart from its surroundings; the 'subject', in this view, is locked inside the mind and everything, including its own body, is external to it. So it is this sort of understanding that leads us to see language, perhaps necessarily, as a means whereby an external reality is represented to an internal 'subject'. It is, in fact, much like the parable of the cave in Plato's *Republic*. Consciousness here is trapped within the cave, only able to understand the world outside via the shadows on the wall. The things themselves, whose

shadows it sees upon the wall, are *outside* and, for this reason, consciousness must resign itself to living in a world of shadows/representations.⁴ So it is because consciousness is, in this view, on its own and independent of the world that Taylor (1993) refers to it as "monological".

But with the construct of 'habitus', Taylor (1993) suggests that we make the move toward a *dialogical* notion of consciousness. In a sense, we abandon the perspective on consciousness that originates with the so-called "modern epistemological tradition" of Descartes and Locke. And in its place, we pick up on a tradition associated with philosophers such as Heidegger, Merleau-Ponty and, most importantly here, Wittgenstein, in which the agent of action is understood to be, fundamentally, immersed in social practices.⁵ The 'subject', in this view, is not a being who contemplates a world but, rather, "a being who acts in and on a world" (Taylor, 1993: 49) and, for this reason, understanding is itself situated within these same sorts of social practices. Or in Taylor's words,

[t]o situate our understanding in practices is to see it as implicit in our activity, and hence as going well beyond what we manage to frame representations of. ... This understanding is more fundamental in two ways: first, it is always there, whereas sometimes we frame representations and sometimes we do not, and, second, the representations we do make are only comprehensible against the background provided by this inarticulate understanding. Rather than representations being the primary locus of understanding, they are ... islands in the sea of our unformulated practical grasp on the world. (Taylor, 1993: 50; cf. Bauman, 1993: 158)

And as he goes on to say, this notion of understanding ascribes a great deal more significance to the role of background. In brief, it acknowledges the unarticulated understandings we acquire through our immersion in a social setting that are, despite their being unarticulated, integral to our proper functioning within that setting.

⁴ I recognize that this is merely one of many ways in which one might understand this parable.

⁵ See also Rorty (1991) for an overview of this tradition. If nothing else, it should be noted that it is the younger Heidegger's writings that are most pertinent to the discussion at hand. Like Wittgenstein, Heidegger appears to have had a change of heart in his later years. However, the two seem to have had opposite trajectories in this regard.

So in the simplest of terms, what this focus upon practices achieves is a "dethroning" of representations in understanding human activity—it is to see the provision of representations as merely a subset of the multitude of social practices we engage in as agents and to recognize that a great many practices transpire without there being any such act of representation. As we know, most of us are capable of traveling through a familiar room in the dark without having to imagine the contents of that room. And what this should reveal is that our understanding is, in many regards, embodied (e.g., Bourdieu, 1981). We are not able to navigate the dark room because we are "processing" representations of it but, rather, because our body "knows" the room as a result of having inhabited it for an extended period of time.

And just as our body might "know" a room, our body has also acquired a sense of our 'selves'; i.e., our sense of our 'self' is, to a large degree, also embodied. When we approach an other with our head bowed, or when we choose to stand at some distance from whomever it is we are speaking with, we are revealing a level of understanding that may never have been articulated for us or by us— a form of non-conscious understanding having to do with our sense of our 'self' *in relation to* an 'other' (see Bauman, 1993). And what this allows us to see is that our understanding is considerably more complex and varied than is indicated by our utterances alone.

So by speaking of 'habitus', Bourdieu is trying to capture and convey these multifarious forms of understanding that allow us to make our way in the world. Nevertheless, for understandable reasons (see Bourdieu & Wacquant, 1992), Bourdieu makes no attempt to explain what, exactly, 'habitus' is. Instead, he describes it in rather cryptic terms that are more akin to a riddle than a definition. In *Outline of a Theory of Practice*, for example, he suggests that:

The structures constitutive of a particular type of environment (e.g. the material conditions of existence characteristic of a class condition) produce *habitus*, systems of durable, transposable *dispositions*, structured structures predisposed to function as structuring structures, that is, as principles of the generation and structuring of practices and representations which can be

objectively "regulated" and "regular" without in any way being the product of obedience to rules, objectively adapted to their goals without presupposing a conscious aiming at ends or an express mastery of the operations necessary to attain them and, being all this, collectively orchestrated without being the product of the orchestrating action of a conductor. (Bourdieu, 1977: 72)

So recognizing that many may be reluctant to invest the time trying to "solve" Bourdieu's riddles, it is worth trying to explicate what Bourdieu might be trying to convey in dense passages such as that just given..

Habitus

As I understand him, Bourdieu is making explicit that 'habitus', at least as he conceives of it (see Bourdieu, 1985), is an emergent phenomenon; i.e., it is not something we are born with or, if we are, it is effectively *tabula rasa* when we arrive in the world. For in his view, it is the "structures constitutive of a particular type of environment" that come to be imprinted upon the habitus. However, it is not the "environment" in any pure phenomenological sense, but rather the particular structures and relations through which we come to know that environment that are imprinted upon it. And going further, in speaking of "structured structures", Bourdieu is acknowledging that these structures are themselves structured in the sense that they are encountered within a particular community of practices; i.e., he is noting that there is a structure to the manner in which these structures can be combined and sequenced. But what he is also saying is that, once internalized, these "structured structures" become "structuring structures" in that they "predispose" us to some "practices and representations" rather than others; they make some actions, and some combinations of actions, more probable than others. And it is in this sense that we can say that they are both "regulated" and "regular" without also saying that they are "the product of obedience to rules."

Also important here is Bourdieu's assertion that these practices are "collectively orchestrated without being the product of the orchestrating action of a conductor." For

what makes this possible is the fact that the practices thus structured by the structured structures of the habitus are, fundamentally, *social* practices; i.e., each is a response to a particular set of practices and, thus, to the 'subjects' associated with those practices. And in addition, each anticipates the practices that might follow from it and, thus, the 'subjects' associated with the practices that come after it (Bakhtin, 1986). So in this sense, no practice and, as a result, no 'subject' exists in isolation. Practices are, in this view, fundamentally dialogical in character (Bakhtin, 1986; Taylor, 1993). It is for this reason that no "conductor" is necessary, whether that be an internal consciousness guiding our thoughts, or an external supervisor, coordinating our actions with those of others. As described by Bourdieu, the "orchestration" is inscribed *in* the practices. And as a result, the 'subject' is not understood to be an ever-present 'monological consciousness' *selecting* one practice from among the available alternatives (cf. Williamson, 1975). Rather, the 'subject' is a 'habitus' *predisposed* to engage in one of these alternatives.

Habitus is, then, the set of resources we have available to us for social interaction, whether that be with others or with our 'selves' (Giddens, 1984). But as I have tried to make clear, it is not a set of resources that a monological consciousness might draw upon at will. In this view, there is no entity apart from these resources that might be called 'consciousness'—no 'self' that might have the "will" to draw upon these resources. Instead, *the resources of habitus are here understood to be constitutive of what is, fundamentally, a dialogical consciousness.* But even so, many if not most of the social practices we engage in are, in this view, carried out in a non-conscious manner. So unlike cognitive perspectives, there is no assumption here that there is some sort of "information processing" continually transpiring somewhere in the mind or, alternatively, that this is some sort of "mindless" enactment of internalized scripts (cf. Gioia & Poole, 1984; Lord & Foti, 1986). Instead, habitus is a set of structured structures—structured in terms of particular communities of practices—predisposing us to engage in some social practices as opposed to others which, if

anything, should us to speak of the mind-*full*-ness of organizational behavior (cf. Ashforth & Fried, 1988). And because *our thoughts are understood here to be social practices internalized*, this implies that it is habitus that predisposes us to think in the particular ways that we do and, to some degree, about the particular subjects that we do.

Unlike the cognitive perspective in which we are encouraged to speak of behavior as though the inescapable result of either rational calculation or mindless mimesis, invoking the construct of habitus encourages us to speak instead of objective probabilities. For habitus is, in effect, a repertoire of social practices— social practices possessing an 'addressivity' and a 'responsivity' (Bakhtin, 1986). By this I mean that, for each social practice, there is a limited range of practices with which to respond and, thus, each social practice is also a response to a limited range of possible predecessors. So in this view, social reality is an endless chain of social practices producing particular outcomes depending upon the particular combinations of practices and the particular settings involved. And habitus is, in this sense, a sort of internalized set of probabilities that has resulted from our direct or vicarious⁶ immersion within these streams of social practices. Nevertheless, according to Bourdieu (1977: 78), "[u]nlike scientific estimations, which are corrected after each experiment in accordance with rigorous rules of calculation, practical estimates give disproportionate weight to early experiences." The reason for this is that the structures of the habitus, once acquired, become "the basis of perception and appreciation of all subsequent experience." Or as he says elsewhere (Bourdieu, 1990: 60),

[t]he *habitus* which, at every moment, structures new experiences in accordance with the structures produced by past experiences, which are modified by the new experiences within the limits defined by their power of selection, brings about a unique integration, dominated by the earliest experiences, of the experiences statistically common to members of the same class.

⁶ Examples of this would be reading about others' experiences or, perhaps more significantly, imagining ourselves having these experiences.

And so it is not only the particular repertoire of structures, but also the order in which they are acquired, that produces the particular sets of probabilities through which we come to "know" particular individuals (cf. Nehamas, 1985).

So to return to my earlier point, consciousness in this view is more of a *capacity* than what we actually are. When confronted with a situation where our unarticulated "understandings" are inadequate, or when given some reason to re-assess our usual responses, we might say that we possess another set of practices with which to contemplate particular chains of events. So it is not that we *are* our consciousnesses but, rather, that we have the capacity for conscious thought as a result of a particular set of practices inscribed upon our habitus, most notably, the set of discursive practices. To reiterate, then, consciousness is *not* ever-present. Rather, consciousness is merely one set of social practices among the many we engage in as we go about our everyday activities. And as with all of our other practices, the discursive practices of consciousness are situated amidst a plethora of other practices. For this reason, individual behavior cannot be accounted for solely in terms of the individual. Instead, it must be accounted for in terms of the socially structured structures of the habitus that are acquired through our immersion within particular communities of practices.

To briefly sum up, what the above implies with respect to OB research is that we should, in effect, be exploring that particular repertoire of practices that might be thought of as constituting the 'organizational habitus'. And to some degree, this is what we have been doing all along. However, what differs here is that in light of the discursive turn, and in light of Bourdieu's formulation of habitus, organizational practices need not be thought of as the conscious decision of a highly rational actor, nor need they be thought of as the unavoidable product of cognitive mechanisms. With respect to the unpredictability of the former, we need only agree with Bourdieu (1977: 85) that "it is impossible for *all* members of the same class (or even two of them) to have had the same experiences, in the same

order." And with respect to the certainty of the latter, to acknowledge as he does that "[t]he shift from the highest probability to absolute certainty is a qualitative leap out of proportion to the numerical difference" (Bourdieu, 1990: 99) and, thus, far, there are few human behaviors that seem to occur with absolute certainty.

So by changing the subject of OB from the 'subject' to habitus, we find a means of accommodating the variability of organizational behavior. In brief, we find a means of exploring routinized behaviors without reducing organizational agents to cultural dupes and, conversely, we find a means of exploring conscious decision making without denying the influence of history and context. But perhaps most importantly, by seeking to explicate the 'organizational habitus', OB's distinctive competence will never be in doubt (cf. Cappelli & Sherer, 1991). In the simplest of terms, our competence will be knowledge of the distinctive fashion in which habitus is structured through its immersion in organizational settings. And given the prevalence of organizational influences (e.g., Deetz, 1992; Shotter, 1993), it will be a competence in which a great many, including our colleagues in psychology, will be interested.

EPILOGUE

For the field of micro OB, there is probably no better barometer of the field's health than the overview provided every couple of years in *The Annual Review of Psychology*. And sadly, the past few reviews have not been that encouraging. For example, without wanting to read too much into this, surely there is something telling in the fact that the most recent review chose to more or less bypass micro issues and focus, instead, on the macro side of OB (Wilpert, 1995). But even if we accept the author's defense of this strategy, there is no sidestepping O'Reilly's (1991) conclusion that OB has more or less entered a fallow period. And lest there be any doubt, in their subsequent review in 1993,

Mowday and Sutton (1993: 196) state that their "review of the five traditional areas covered in O'Reilly's (1991) chapter) also revealed that novelty and intellectual excitement were largely absent." Harsh words, without a doubt, but particularly harsh when coming from within the discipline.

But in addition to their harsh reviews of past research, what unites these reviews is their recognition of the theoretical excitement within macro OB in recent years. And perhaps for this reason, what they also have in common is a belief that the future of micro OB lies in providing a bigger role for context, a domain traditionally reserved for those on the macro side of the coin. But thus far, what has been lacking is some indication of how we might go about accomplishing this.

So with this article, I have tried to inject some of this much-needed theoretical excitement into OB. What I have tried to do is show that the same philosophical currents that are influencing macro research, promise to be equally influential within micro research. The question, thus, is not "Whether?" but, rather, "When?" On the one hand, should we choose to wait for our colleagues in psychology and social psychology to "translate" these philosophical developments into familiar language, it will likely be several more years before any intellectual excitement takes hold within our field. But if, on the other hand, we are willing to wade into the philosophical waters on our own, as I have tried to do herein, then this excitement may come about considerably quicker. Nevertheless, tempting as it is to conclude that the choice is ours as to when we embrace these new understandings, it would be a mistake to conclude that our timing is of no consequence. For as those doing macro research follow through on the implications of these ideas, they are encroaching more and more upon topics traditionally reserved for micro researchers. And should this continue, it may not be just our distinctive competence that is called into question (e.g., Cappelli & Sherer, 1991) but, instead, our entire *raison d'être*.

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Chapter Three
RELATIONALITY

Michael K. Mauws

Perhaps the immobility of the things that surround us is forced upon them by our conviction that they are themselves and not anything else, by the immobility of our conception of them.

— Marcel Proust, *Remembrance of Things Past*

An almost unquestionable truth for me is that how we talk about our lives has an enormous impact on how we live them. So what I would like to do in this article is show how our way of speaking affects what we do as researchers of organizational phenomena. Specifically, what I hope to show is that the manner in which we talk *about* our research has significant effects upon both how we do our research and what we consider to be researchable topics. And taking this further, what I would hope to show is that, quite often, the seemingly insurmountable obstacles we encounter in our research would not arise if only we had alternative ways of talking about our research.

Toward this end, I would like to suggest that thus far we have really had only one way of speaking of our research and this is in the terms given to us by Burrell and Morgan (1979) in their *Sociological Paradigms and Organizational Analysis (SP&OA)*. Although there have been some variations put forth in the interim (e.g., Astley & Van de Ven, 1983), for whatever reason, the metatheoretical furrows that demarcate our field appear to be those originally laid down by Burrell and Morgan (1979). And in light of this, it is important that we recognize that these furrows have had significant consequences. Perhaps most importantly, the divisions thus created have provided each of us with an identity for both ourselves and our colleagues. As a result, they have also affected what each of us reads and, with it, whom we choose to work with. So it is in this sense, then, that I am suggesting that how we talk about our research affects the research that gets done.

The immediate response to this will no doubt be that, rather than *creating* divisions within our field, Burrell and Morgan (1979) have simply *described* divisions that already existed within it. It will be said that if subsequent research is slotted into these categories, if researchers continue to apply these categories to themselves, then this is probably the

best indication we could ask for that they may have "gotten it right". After all, without an alternative set of categories to be compared to, how could we ever know otherwise? And even if it were possible for us to know this, in the absence of an alternative, what would we do if we did in fact conclude that Burrell and Morgan (1979) had *not* gotten it right?

And yet, the point I wish to make has little to do with "getting it right". Rather, it is simply about acknowledging that our ways of talking influence what we do and what we subsequently say. For even if we lack an alternative way of speaking, by recognizing the influence of our current mode of doing so we can make ourselves aware of potential biases and possible pitfalls. In this regard, Hugh Wilmott's (1993) discussion of *SP&OA* is an excellent example. His goal was not to supplant it but, nevertheless, to draw our attention to its dichotomous nature. By doing so, he was trying to tell us that we would always feel compelled to choose *either* subjectivism *or* objectivism, to single out *either* subject *or* object, so long as we continued to speak of our research in these terms. However, as I understood him, he was *not* saying that there was anything "wrong" with talking about our research in these terms. Instead, he was simply telling us that should we find this to be an awkward way of speaking, rather than changing the way we do our research we may, instead, want to change the way we speak.

So what I would first like to propose in this article is that some of us truly are having difficulties talking about our research in the terms provided in *SP&OA*. This observation is borne out in an ongoing dialogue that appears to have reached "a point of decreasing (or even increasingly negative) marginal utility" (Nord & Connell, 1993: 109). However, as I note below, the discussion thus far has centred around a rehabilitation of Kuhn's (1970) concepts of 'paradigm' and 'incommensurability', leaving the vocabulary of *SP&OA* more or less intact. So for this reason I have adopted the opposite strategy; i.e., I have taken the notions of 'paradigm' and 'incommensurability' for granted and, instead, asked some questions of the mode of description provided in *SP&OA*. And by doing so, the

conclusion I have been led to is that, rather than four paradigms – Interpretive, Functionalist, Radical Humanist, and Radical Structuralist – or even two – Subjectivist and Objectivist – what Burrell and Morgan (1979) have provided is, in fact, a single paradigm which I suggest we call the Duality paradigm.

Thus, the bulk of this article will be devoted to articulating what, exactly, I mean by “the Duality paradigm”. More importantly, it will be devoted to a discussion of how this singular paradigm has influenced our research and our ways of talking about it. But recognizing that to some degree this was the aim of Wilmott’s (1993) discussion, I hope to move us forward by also proposing an alternative paradigm – Relationality – and juxtaposing it with that which I am calling the Duality paradigm.

However, in doing so my intention is not to replace the language of Duality with that of Relationality. For as I see it, the language of Duality encourages us to attach ontological primacy to *things*, whether they be ‘subjects’ or ‘objects’. And so long as this provides us with the answers we seek, I can sincerely say that I see no reason why we should speak in any other terms. Nevertheless, there may be times when, instead of things, we wish to have *relations* as our ontological ground-zero, and it is in instances such as this that we may be better off adopting the vocabulary of Relationality. So rather than being about “getting things right” *per se*, I would like to think instead that that which follows is aimed at providing researchers with a choice of vocabularies that permits an even greater number of us to “get things right”.

PARADIGMS & INCOMMENSURABILITY

Of all the oppositions that artificially divide social science, the most fundamental, and the most ruinous, is the one that is set up between subjectivism and objectivism.

– Pierre Bourdieu, *The Logic of Practice*

In their *Sociological Paradigms and Organizational Analysis* Burrell and Morgan (1979) draw upon Kuhn's (1970) discussion of the philosophy of the natural sciences in suggesting that research within organizational analysis can be described in terms of four 'incommensurable paradigms'. By "paradigm" they mean a particular way of seeing the world; by "incommensurable" they mean that each paradigm constitutes a fundamentally different way of seeing that world – so different that descriptions of the world within one paradigm are likely to be non-sensical within the other. So when Hassard (1991), for example, uses this vocabulary to describe his investigations as having drawn upon four different paradigms, Parker and McHugh (1991; see also Jackson & Carter, 1991) correctly note that this is simply not possible: paradigms are *by definition* incommensurable. Similarly, when Willmott (1993) draws upon this vocabulary to describe his work as being both objectivist and subjectivist (see also Adler & Borys, 1993), others once again disagree on the grounds that these are incommensurable perspectives (Jackson & Carter, 1993; cf. Willmott, 1993a).

So regardless of whether someone might actually be "right" here, what is clear to me is that we are having difficulties. For on the one hand, I must confess that the so-called multi-paradigmatic research is quite convincing in its conclusions, and its defenders quite effective in arguing its multi-paradigmatic nature (see also Gioia & Pitre, 1990; Lee, 1991). But on the other hand, in invoking both Kuhn (1970) and Burrell and Morgan (1979) as they do, I cannot help but conclude that those who object seem quite justified in doing so. So at least at first glance, it would appear that the terminology of *SP&OA* has permitted the expression of two positions that are diametrically opposed to each other.

For those seeking a way out of this paradox, the assumption thus far has been that it lies in the notion of 'incommensurability'. By this I mean that those proclaiming their work "multi-paradigmatic" have sought acceptance by proposing the abandonment of the so-called 'incommensurability thesis'. For needless to say, if they *could* establish that their

work truly is multi-paradigmatic, then the future of this thesis would most definitely be in doubt.

However, it would appear that those who are proposing this solution may have underestimated the tenacity with which their adversaries might cling to this much-treasured thesis. For rather than acknowledging the multi-paradigmatic nature of the research in question, and with it the end of 'incommensurability', the latter have instead indulged in ever-deeper exegeses of Kuhn (1970) and Burrell and Morgan (1979). Their intention, obviously, has been to buttress the notion of 'incommensurability'. And as both camps dig themselves further and further in, they do seem to prove the incommensurability thesis but, tellingly, only through their own inability to communicate.

Nevertheless, if I had to choose sides in this standoff, I suspect it would be with those clinging to their well-thumbed copies of Kuhn's (1970) *The Structure of Scientific Revolutions*. For as I understand it, Kuhn (1970) is not suggesting that paradigms are naturally occurring phenomena that actually are incommensurable. Rather, he is saying that we might think about using this term "paradigm" to refer to a theoretical perspective that seems to be incommensurable with some other perspective. However, the implication I would draw from this is that, if two perspectives that were previously thought to be incommensurable no longer seem to be so, the difficulty is probably with what we are calling "paradigms" and not with our understanding of 'incommensurability'. In other words, if two or more of what we are calling "paradigms" can be said to resonate within the same piece of research, it does not imply the end of 'incommensurability'. However, it does imply that perspectives of this sort are probably not what Kuhn had in mind in speaking of 'incommensurable paradigms'.

Thus, if there is a way out of this tiresome debate, it would seem to lie with what we are calling "paradigms". Whether it be the four quadrants of Burrell and Morgan's (1979)

theoretical map, or the more common distinction between subject and object, this putative paradox of multi-paradigmatic research would seem to indicate that we should think twice about what it is we are calling paradigms. For if there is any incommensurability here to speak of at all, it would appear to be between those whose research can be adequately described in the terminology of *SP&OA* and those whose research cannot. And what this would imply is that it is not the individual theoretical quadrants, nor is it subjectivism and objectivism that are properly referred to as paradigms. Instead, what this would imply is that it is Burrell and Morgan's (1979) entire framework which we should be thinking of as a paradigm. Furthermore, if we are finding that some research seems non-sensical within it – e.g., multi-paradigmatic research – then this is probably our best indication that there is a need to articulate an alternative paradigm.

So the question at this point is, in what sense could we conceive of *SP&OA* as a paradigm unto itself? Alternatively, we might ask what is it that is common to each of the perspectives articulated therein and yet not common to the research that fits none or more than one of these perspectives? If we can establish this, then presumably we will have some indication of where we might look for an alternative paradigm. So in order to answer this, an effective strategy may be to trace this work back to the critical juncture at which the subject/object distinction is brought into being. By examining their sources we may discover that others have already encountered and dealt with the difficulties being addressed herein.

Beyond Realism and Relativism

Sociological Paradigms & Organizational Analysis (Burrell and Morgan, 1979: 1-9) begins with an introduction to the basic concepts of metatheory. The name "metatheory" literally means "above" theory. But in describing it this way, all we are saying is that metatheory is theory about theory rather than theory about some aspect of the world

(Morrow, 1994). When we ask questions about metatheoretical foundations what we are really saying is that we are interested in knowing what assumptions we would be making in adopting a particular theoretical position; what is it, in other words, that is presupposed by a particular perspective. With respect to social science research, for example, we are often interested in the ontological, epistemological and methodological assumptions, as well as the implications for human agency (Burrell & Morgan, 1979). And by focusing on these assumptions as opposed to any others, we are saying that we find it useful to talk about our theories by what they presuppose with respect to the nature of the world, the nature of knowledge, and methods for acquiring knowledge, respectively. In addition, we often find it useful to ask whether they portray human activity as being determined from within or from without (e.g., Reed, 1988).

These concepts are introduced by Burrell and Morgan (1979) in order to attach a pair of alternatives to each. Specifically, they suggest that theories can be described in terms of a 'realist' or 'nominalist' ontology, a 'positivist' or 'anti-positivist' epistemology, a 'nomothetic' or 'ideographic' methodology, and a 'determinist' or 'voluntarist' conception of human nature. Nevertheless, instead of viewing these as four dimensions providing sixteen possible combinations, they choose instead to collapse them into two positions which they label 'objectivism' and 'subjectivism'. Objectivism they identify with realism, positivism, determinism and nomothetic modes of inquiry, while subjectivism is identified with nominalism, anti-positivism, voluntarism and ideographic modes of inquiry. So it is here, then, that the Duality paradigm comes into being.

In articulating the positions they do, Burrell and Morgan (1979) are drawing upon a philosophical discussion with a very long history. But within philosophical circles, the distinction between 'objectivism' and 'subjectivism' is more often discussed in terms of "materialism" and "idealism" (see Adler & Borys, 1993). That said, it is worth noting that it was at the turn of the century that this putative tension between realist and idealist

positions dominated philosophical discussions (Rorty, 1991). However, in light of the 'linguistic turn' (e.g., Rorty, 1992; Winch, 1958; Wittgenstein, 1958; see also Mauws & Phillips, 1995; Van Maanen, 1995),¹ philosophers today seldom juxtapose reality with the mind in the manner suggested by this distinction. Instead, what they are more likely to discuss is the relationship between reality and language (Rorty, 1991: 2).

Instead of worrying about whether reality might be "mind-dependent" – the idealist position – or independent of the mind – the realist position – philosophers have now shifted their attention to the relationship between linguistic statements and the nonlinguistic elements that are the world. What they are interested in, in essence, is the possibility of our making "true" statements, by which they mean statements corresponding to something in the world. So-called realists clearly have no doubt that this is possible and would, in fact, argue that we make such statements all the time. Nevertheless, for others it is not entirely clear that this is so and, for this reason, those who adhere to this position have come to be known as anti-realists.²

But according to Rorty (1991), this distinction between realism and anti-realism is somewhat ambiguous and can sometimes be misleading. For what is being called "realism" today is not identical with that which had previously been juxtaposed with idealism. Rather than being about any distinction between the world "out there" and the world "inside" our heads, the important distinction being drawn in contemporary debates has to do with whether or not we can *represent* the world. That is, the debate is about whether our representations of the world do, in fact, somehow correspond to it (Manning,

¹ Rorty (1991: 50) attributes the term to Gustav Bergmann.

² In explaining the distinction between realism and anti-realism, Rorty (1991: 3) defers to Dummett, with whom he credits this distinction: "Realism I characterize as the belief that statements of the disputed class possess an objective truth-value, independently of our means of knowing it: they are true or false in virtue of a reality existing independently of us. The anti-realist opposes to this the view that statements of the disputed class are to be understood only by reference to the sort of thing which we count as evidence for a statement of that class" (Dummett, 1978: 146).

1979; Rorty, 1979). So in order to avoid the possibility of confusion, Rorty suggests we speak in terms of "representationalism" and "anti-representationalism" as opposed to realism and anti-realism. By doing so, we avoid implying that the debate has something to do with whether or not there is something we can call "reality" and, instead, draw attention to the actual source of disagreement; i.e., the possibility of representing the world in language.

So what we find, then, is that within philosophical discussions the critical cleavage has shifted from ontological grounds – i.e., the nature of reality – to epistemological grounds. More specifically, what is at stake among philosophers is not so much what might be meant by 'world' or 'reality' but, instead, what might be meant by 'truth'. For the representationalists, true statements are those that correspond to something in the world. Statements can, in their view, be proven or falsified through comparison with that which is the world. But for anti-representationalists, this is absurd. From their perspective, we have no way of gaining unmediated access to the world and, as a result, it is futile for us to define 'truth' in terms of correspondence with it. For this reason, 'truth' is better defined in terms of *coherence* among statements, for at least this is something we are capable of determining.

But at this point we might ask what the connection is between the so-called "representationalist debate" in philosophy and our own interest in determining whether we might interpret *SP&OA* as indicative of a single paradigm. In other words, is there something we can learn from the fact that the philosophers have more or less abandoned the materialism *versus* idealism debate and are now focusing their attention upon issues of representation?

Most definitely. For philosophers have not so much abandoned this debate as they have put it off until this "preliminary" issue of representation has been sorted out.

Presumably, if all could agree that it is possible to unproblematically represent the world in language, then at this point the materialism/idealism distinction would once again come to the fore. But in the interim, it is only those philosophers that have "chosen" representationalism that can then go on to choose 'materialism' or 'idealism' or, in Burrell and Morgan's (1979) terminology, 'subjectivism' or 'objectivism'. And so, despite their putative differences, what unites subjectivists and objectivists and juxtaposes them with the "others" is simply the shared belief that they can represent their respective worlds in language. In other words, both subjectivists and objectivists ascribe to a correspondence conception of truth; they believe that their descriptions of the 'world' do, in fact, correspond to some extant world, whether it be "in here" or "out there". So incommensurability, to the degree that it exists, is not between objectivists and subjectivists but, rather, between both these groups and those that deny the possibility of representing the world.

What is being suggested here, then, is that incommensurability applies to differing conceptions of truth. And in retrospect, this makes a great deal more sense. For if incommensurability refers to incomparable and non-combinable systems of knowledge, and if knowledge is in some sense understood to be that which is 'true', then it would follow that divergent perspectives on what counts as 'truth' would be likely to produce incommensurable systems of knowledge.

So despite their disagreements over the ultimate "location" of the world, the fact that subjectivists and objectivists alike agree that what they are describing is, in fact, the world would seem to imply that their positions are not incommensurable. Both groups agree that the world is best described in terms of subjects and objects and the disagreement, to the degree that there is one, appears to be over who inhabits whose world. But what these accounts *are* incommensurable with are those descriptions that refuse to attach ontological

primacy to *either* subject *or* object and, in so doing, deny the existence of both. It is these accounts, then, that I wish to identify as indicative of the Relationality paradigm.

A TALE OF TWO PARADIGMS

The suggestion thus far is that it is the difference between *correspondence* and *coherence* conceptions of 'truth' that differentiates the Duality and Relationality paradigms, respectively. Nevertheless, in practice the differences between the two are seldom expressed in these terms. Instead, their differing conceptions of 'truth' are manifested in the particular ways of speaking associated with each and, in particular, the metatheoretical assumptions that each encourages us to make. For this reason, what I would like to do in this section of this article is to delineate what, exactly, I wish to identify by these two paradigms by contrasting their respective metatheoretical positions.

Toward this end, Table 1 outlines what I feel to be the respective metatheoretical positions of these two paradigms. As you can see, the manner in which I intend to contrast the two is in terms of 'truth', which I have already discussed above, as well as their ontological, epistemological and methodological positions. In addition, I also intend to look at how each encourages us to define our research problems. Toward this end, each of these topics is addressed in a corresponding sub-section below.

But before I proceed, there are two points in need of reiteration at this time. First, it needs to be re-stated that what I am trying to explore here are two different ways of talking about our research. So in saying that I intend to contrast their 'positions' or 'assumptions', what I am really saying is that I want to compare the manner in which each encourages us to talk about our research. And this, in turn, leads me to my second point of reiteration which is that, thus far, we have only had the one way of talking about our research. For this reason, it is likely that some research heretofore described as either

	<i>DUALITY</i>	<i>RELATIONALITY</i>
TRUTH	Correspondence	Coherence
ONTOLOGY	Things	Relationships
EPISTEMOLOGY	Atomistic	Holistic
METHODOLOGY	Syn/Diachronic	Praxeological
PROBLEMATIC	Change	Stability

Table 1: Paradigmatic Differences

subjectivist or objectivist may not be labeled such in the presence of an alternative vocabulary. So what I am effectively saying is that, if my portrayal of the Dualist positions of subjectivism and objectivism would seem to preclude research that we presently associate with these positions, we should ask ourselves first whether this research is not better described within the Relationalist paradigm. If not, then it is at that point that I may have to acknowledge that the vocabulary proposed herein is not as useful as I might have hoped.

Ontology: The Nature of Reality

The ontological distinction between the Duality and Relationality paradigms is best described in terms of *things* and *relations*. By this I mean that the Duality paradigm encourages us to speak in terms of "things entering into relationships with other things", whereas the Relationality paradigm encourages us to speak in terms of "things emerging or coming into being through relationships". So as can be seen, for a Dualist there is no point talking about 'relations' until we have familiarized ourselves with the things entering into these relations. Conversely, for a Relationalist, it is only through relations that these things exist for us and, thus, to explore reality in terms of the latter is to put the "empirical cart" before the "ontological horse". This, then, is the essence of the discordance between these two paradigms.

In order to explore this difference further, it is useful to draw a distinction between 'world' and 'reality'. If we do so, "the world" would refer to the physical entity of which we are a part; to that which, at least in some sense, we are trying to cope with or live within through our seeking knowledge of it. Nevertheless, until such time that we can confidently assert that we *know* the world in its entirety, it is difficult to fathom how we might actually speak of the world *per se*, i.e., how could we know what is meant by "the world"? So in light of this difficulty, we can use the term "reality" to fill the gap. Thus understood, 'reality' would be the world as we know it; it would be what we are *actually* thinking of when we think we are thinking of something called "the world". This is not to deny that 'world' and 'reality' are inextricably linked; rather, it is to recognize that, despite their overlaps, the two need not be the same (Bhaskar, 1989; Marsden, 1993; Rorty, 1989; 1991).

However, if we speak in terms of the Duality paradigm, this distinction between the world and reality serves little or no purpose. Because it speaks of its truths (reality) as though corresponding with the world, world and reality are one and the same; they are, so to speak, always in sync. So objectivists, for example, speak of a singular world/reality "out there" inhabited by all of us. For them there is only one world/reality to know and, working together, we will eventually know that world/reality in its entirety. For subjectivists, on the other hand, the world/reality is thought to be "in here"; it is, so to speak, in the minds of individuals. Thus, there are as many worlds/realities as there are individuals and, in each case, world and reality are one and the same. In light of this, the aim of subjectivist research is, in effect, to determine what happens when worlds "collide". The important point here, though, is that either way, whether we speak in terms of subjectivism or objectivism, because it is assumed that our representations of the world (our 'reality') correspond to the world, world and reality cannot help but be identical.

But when we problematize the possibility of representing the world as the Relationality paradigm encourages us to do, world and reality need no longer coincide. We can still speak of a single "objective" world common to all but, as was discussed above, we acknowledge that we can never really know that world.³ So 'reality', rather than being a model of the world, is understood here to be a *strategy* for living in that which is the world (Bourdieu, 1986). Our theories and facts, in this view, are simply metaphors and tropes that allow us to survive in a world that continually eludes us (Manning, 1979; Rorty, 1989). When we speak of falsifying or proving a theory, we are talking about finding metaphors better suited to our purposes. For as Kuhn (1970) has effectively shown, we are not converging upon a picture of the world itself. Thus, rather than speaking of getting to *know* the world, the vocabulary of Relationality encourages us to speak of finding better ways to *live* in the world.

For example, in this view Newton's vocabulary is a strategy that is, for most of us, more than adequate for living in the world. This despite the fact that the latter could be more "accurately" described using Einstein's vocabulary. Or more specifically, this despite the fact that the vocabulary originating with Einstein would allow us to accomplish even more than we are able to with that of Newton. However, in the language of Relationality, Einstein's vocabulary is no more a picture of reality than is Newton's; both are merely strategies for living amidst whatever it is that is the world. Thus, the decision as to which of these vocabularies we should adopt is not determined according to any correspondence with the world. If this were the case then, presumably, we would all speak in Einstein's terms. But because it is not, it is that which we hope to accomplish that determines the vocabulary we adopt.

³ The primary reason for this is that knowledge is itself relational. If nothing else, it is this that we have learned from the Structuralism of Saussure and the French poststructuralists (e.g., Derrida, Foucault, Lacan).

The immediate and important implication of this example is that, within the terminology of Relationality, it is at least possible for multiple realities to coexist. However, both vocabularies in the example above are attempts to live within the same world. Nevertheless, because they provide different ways of doing so, they constitute fundamentally different realities for the individuals that adopt them.

So it is precisely this, then, that differentiates Relationality from the positions associated with Duality. Whereas objectivism posits a singular reality corresponding to a singular world, subjectivism posits multiple realities, each of which corresponds to its own subjective world. And in between these two is Relationality which posits multiple realities, while still maintaining that all are merely strategies for living within the one and only world there is. Thus, contrary to subjectivism, in which realities are the private possessions of individuals, within Relationality these realities are shared, negotiated and *inter-subjective* but, nevertheless, constrained by the affordances of whatever it is we are calling the world.

In many regards Relationality resonates with what is known as 'critical realism' (e.g., Bhaskar, 1989; Marsden, 1993). For within the latter, we are encouraged to distinguish between the 'real', the 'actual' and the 'empirical'. With this distinction in place, it is only the physical matter of which the world is composed that would be classified as 'real'. For as Bhaskar (1986: 106) notes, "the world consists of things, not events." Nevertheless, as a result of *actual* events within the *real* world, we are presented with the *empirical* facts from which we infer the presence of the world and the things of which it is composed. But at the same time, because the possibility exists that events might also be transpiring in such a manner so as to produce no empirical effects (Marsden, 1993) – i.e., events counteracting each other or remaining undetected with current instrumentation – we are forced to acknowledge that any description of the world that relies solely on empirical observation is likely to be incomplete (cf. Donaldson, 1992). Tying this into the discussion above, the

language of Relationality is merely intended discourage us from treating the empirical (reality) as though it were real (the world).

So although we may still speak of 'subjects' and 'objects' within the Relationality paradigm, these are no longer understood to be the real things of the world. For as will be recalled, within the framework of Relationality the understanding is that we can never gain unmediated access to the world. For this reason, we have no basis for determining the fundamental entities of which the world might be comprised and, as a result, our only strategy can be to assume that *the world is the one and only thing there is*. Thus, rather than corresponding to things *per se*, within Relationality the terms 'subject' and 'object' are understood to denote particular sets of relations among the attributes of that which is the world.

What this implies is that in the language of Relationality neither the 'subject' of Organizational Behavior (OB) nor the 'object' of Organizational Theory (OT) actually exist (in the specific sense of this term associated with critical realism). That we continue to speak in these terms merely indicates that we have found it effective to do so (Gephart, 1978; 1993; Linstead & Grafton-Small, 1992; Sandelands & Srivatsan, 1993). It indicates that we have found personal names to be an effective means of identifying a particular set of practices and the physical organism with which they are associated. In addition, it indicates that proper names are an effective means of identifying particular sets of organizational activities and the individuals associated with them (Manning, 1979). However, it is the fact that these names do not correspond to some thing in the world that allows us to account for our continuing to use the same names despite changes in that which they refer to. And furthermore, it allows us to see that if we no longer find it effective to speak in these terms, there is nothing compelling us to continue doing so. In brief, it is not the subjects and objects that exist in this view but, rather, the physical effects

and the relations among them which these so-called subjects and objects are serving to identify (Foucault, 1972).

So from this we can see how their respective advocacy of correspondence and coherence conceptions of 'truth' has led to Duality and Relationality having incommensurable ontological positions. Within Duality, it is the so-called things of the world that are taken for granted – that are, as a result, the starting point of any investigation. But within Relationality, to begin here is to use that which is to be explained as a means of explaining it (see Bittner, 1965). In light of this, the suggestion is that the starting point of any investigation must be with the relations that have brought these entities into being. Either way, what I would hope to have made clear is that, regardless of the paradigm and ontological position we adopt, in doing so we commit ourselves to a particular approach to research carrying with it a particular conception of knowledge.

Epistemology: The Nature of Knowledge

Burrell and Morgan (1979) attach the epistemological perspectives of 'positivism' and 'anti-positivism' to objectivism and subjectivism, respectively. But despite the seemingly antithetical nature that their names might imply, what I wish to argue here is that, as described, both positions are inherently *atomistic*. That is, they imply that knowledge is gained in terms of small, bite-sized chunks that can be put together in some meaningful way. By contrast, the epistemological perspective of Relationality can only be described as *holistic*. By this I mean that the assumption is that the parts are only knowable in relation to the whole and, thus, we must know the whole before we can know the parts. For this reason, we find once again that Duality and Relationality point us in opposite directions.

Let us begin with 'positivism'. For as we all know, 'positivism' has had a long history within the natural sciences, and even within the social sciences. Within our discipline, its

popularity appears to have waned somewhat but, nevertheless, there are still some who trumpet its virtues (e.g., Donaldson, 1985; 1992). That said, we are told in *SP&OA* that the objective for those who adopt a positivist epistemology is to "seek to explain and predict what happens in the social world by searching for regularities and causal relationships between its *constituent elements*." The rationale behind this is the belief that "knowledge is essentially a *cumulative* process in which new insights are *added* to the existing stock of knowledge and false hypotheses *eliminated*" (Burrell & Morgan, 1979: 5, emphasis added). What this implies, then, is that somehow we can get these bits of knowledge "right" once and for all or, at the very least, that we can come closer and closer to representing the world in its entirety. Needless to say, this approach is only sustainable if one adopts a correspondence conception of truth as is the case within the Duality paradigm.

So turning now to the epistemological perspective of subjectivism, if there is a sense in which 'anti-positivism' can be truly seen as the antithesis of 'positivism', it might be attributed to its unwillingness to integrate the resultant pieces of its atomistic epistemology: whereas the organizing image of positivism is that of the jigsaw puzzle, slowly but surely being pieced together, the image of anti-positivism presented in *SP&OA* is akin to a pane of glass shattered forever against a concrete floor. More to the point, the belief is that "the social world is essentially relativistic and can only be understood from the point of view of the *individuals* who are directly involved in the activities which are to be studied" (Burrell & Morgan, 1979: 5, emphasis added). For this reason, there is no hope of integration or generalization as there is in the vocabulary of positivism. Instead, the best we can hope for is to gain an understanding (*Verstehen*) of how particular outcomes have come about through an exploration of the subjective realities of the individuals involved.

What I would hope would be clear from this is that it is only if we adopt the Duality paradigm, and with it the tendency to speak in terms of things, that we can then go on to adopt one or the other of these epistemological perspectives. For what both epistemologies encourage us to do is approach the object of our inquiry as though a thing-in-itself: to treat it as independent of the particular context in which it is situated. This is not to say that context is ignored altogether, for both subjectivist and objectivist studies alike do make concerted efforts to incorporate context in their analyses. Nevertheless, the assumption therein is that, through a process of distillation, the *essence* of each subject or object can be revealed. Or stated slightly differently, what is implied is that we would still have something remaining were it somehow possible to drain away each and every contextual influence.

But in the language of Relationality, assertions such as this do not arise. In its terms, this would be described as the "fallacy of misplaced concreteness" (Whitehead, 1926: 64), or what is more commonly referred to as 'reification'. Adopting Thomason's (1982: 1) definition, by the latter I mean "certain cognitive processes by which an unjustified concreteness, autonomy, facticity, impersonality, objectivity and independence is attributed to various elements of experience" (see also Sandelands & Srivatsan, 1993). Thus, from the perspective of Relationality, to attribute an essence to subjects and objects that is above and beyond the context in which they are situated is only possible if one equates these entities with some thing in the world; i.e., if we treat them as things-in-themselves. But having problematized the ability to know and represent the things that are the world, those who adopt the Relationality paradigm have little choice but to adopt or adapt an alternative epistemology.

In this regard, the epistemological strategy that I am associating with Relationality is that of 'holism'. In the simplest of terms, its underlying premise is that what any element "is" or what it "means" is only knowable in relation to the whole in which it is situated

(Bourdieu, 1990; Meyer, Tsui & Hinings, 1993; Nehamas, 1985). However, this perspective is not unique to relationality and, in fact, it originates in the structuralism of Saussure (1983) and Lévi-Strauss (1963; see also Sturrock, 1986). But regardless of its origins or applications, its implication is that there are no essences to be considered; that not only is each element's meaning or identity determined in relation to the whole but, in fact, that each element only *exists* or comes into being in relation to the whole. Thus, there is no such thing as a thing-in-itself, for in this view what we are calling a "thing" is nothing more than the "sum of its effects" (Nehamas, 1985; Nietzsche, 1968). And because the effects associated with a particular subject or object are the product of the set of relations within which it has been identified, "[r]emoving or altering even one element from the whole to which it has been construed to belong destroys both whole and part: it alters what needs to be explained and changes that which might provide the explanation" (Nehamas, 1985: 78). Thus, it is for this reason that the epistemological perspective of the relationality paradigm is fundamentally holistic.

The difficulty within Relationality, then, has to do with demarcating the whole within which the elements in question are thought to be constituted. For if what each element "is" or "means" is only knowable in relation to the whole, then the identification of what we are calling the whole is a critical step in the research process. So in the past the strategy was to effectively posit a single system or totality in which every element was determined (e.g., Saussure, 1983; Lévi-Strauss, 1963). However, approached in this manner, it was then difficult to conceive of some manner in which change might come about; i.e., the meaning or identity of each element seemed to be determined once and for all. So what differentiates the holistic epistemology of Relationality from its structuralist predecessors is the positing of multiple 'fields' (e.g., Bourdieu, 1992), 'discursive formations' (e.g., Foucault, 1972) and other such analytical devices in place of a single 'system' or 'totality'. In addition, these entities are usually now understood to have ambiguous boundaries and,

at least to some degree, to overlap with one another (Dews, 1987). So as I discuss at greater length below, the result of this is that the methodological focus then shifts from the individual elements of a world or totality to the development of the 'fields' or 'formations' that bring these elements into existence (e.g., Powell and DiMaggio, 1991). And as is once again evident, it is this emphasis on the whole as opposed to the pieces that distinguished the epistemology of Relationality from its Duality counterparts.

Methodology: Modes of Inquiry

As we shall see, it is this focus upon things being brought into existence as opposed to their having always existed that most clearly differentiates the methodological strategies of Relationality and Duality, respectively. And as should be apparent, this can be traced back to their respective epistemological and ontological positions. Nevertheless, despite the latter differences, in practice the difference between their methodological strategies is not as pronounced as might be suspected.

Consider that in *SP&OA* we are told that objectivism and subjectivism are associated with 'nomothetic' and 'ideographic' methodologies, respectively. In the simplest of terms, the former refers to those methods aimed at discerning regularities among multiple instances of the same phenomenon, presumably with the intention of revealing the essence of that phenomenon so that we might know it once and for all. Conversely, the latter refers to methods that provide an account of a singular phenomenon by revealing the essences that are the subjective realities of the individuals involved. Thus, within the Duality paradigm, where the essences of subjects and objects are, in effect, mutually exclusive, these two methodologies are also understood to be mutually exclusive. But because the language of Relationality does not present us with a choice between 'subjects' and objects' and, instead, portrays all entities as merely bundles of effects or relations, it may also be possible to see these methodologies as complimentary. However, to see how

this might be so it is useful to remind ourselves that so-called "nomothetic" approaches are often synchronic in nature, whereas "ideographic" approaches tend to be diachronic and to see what this might tell us about their respective results.

In their synchronic form, objectivist investigations tend to take a "snapshot" of the world at a particular point in time. These snapshots, in turn, are examined with the intention of identifying multiple instances of the *same* phenomenon. The reason for this is that each such instance is understood to be a deviation from its naturally given form or essence. Thus, armed with this interpretation, the collection of instances is then interpreted in terms of an underlying 'normal' distribution and it is in this sense that these studies are nomothetic. So without delving into theories of probability, the important conclusion here is that each such entity is to a greater or lesser degree understood to be a deviation from its "normal" form; i.e., from its *naturally* occurring form. So in the most succinct of terms, in investigations rooted in Duality, it is the collection of these "forms" that constitutes the ontologically invariant objects (things) of the world.

But whereas objectivism with its nomothetic/synchronic analyses more or less presents the richness and diversity of lived experience in terms of variations and mutations in the objects that are the world, the diachronic accounts of subjectivist analyses are more likely to attribute this to variations in the respective worlds of the subjects under investigation, and to variations in the specific subjects themselves. In other words, subjectivist analyses are more likely to be diachronic in that they look at a series of events over time and then account for them in terms of the interpretations of those involved, as well as the realities that led them to those interpretations. Thus, deviation is not in the objects themselves but, instead, in the respective realities of the subjects who interact and it is in this sense that these studies are ideographic. Nevertheless, despite the deviation both between subjects and within them over time, there is still an essence implied that can be considered the ontological invariant of subjectivism.

The difference, then, is that in the language of Relationality our categories of analysis are not so much ontological as they are *genealogical* (Marsden, 1993; Nehamas, 1985). By this I mean that, despite their seeming ahistoricism and immutability, even the most robust of categories can be described as though the product of historical events (e.g., Foucault, 1965; 1970; 1978). And in light of this, Relationality encourages both synchronic and diachronic analyses. By relaxing the assumption that what we are dealing with is ontologically given, it seeks to show how the categories of synchronic analyses are brought into being through events best described diachronically and, conversely, how these categories thus constituted are then deployed within subsequent events. In the end, rather than rejecting either or both modes of inquiry, what the vocabulary of Relationality allows us to do instead is transcend the antagonism between the two while preserving the gains associated with both (Bourdieu, 1990: 25).

It is exactly this that Pierre Bourdieu (1973; 1977; 1990) has been arguing for with his so-called "praxeological" mode of inquiry (see Ranson, Hinings & Greenwood, 1980). As he puts it:

In order to escape from the *realism of the structure*, which hypostatizes systems of objective relations by converting them into totalities already constituted outside of individual history and group history, it is necessary to pass from the *opus operatum* to the *modus operandi*, from statistical regularity or algebraic structure to the principle of the production of this observed order, and to construct the theory of practice, or, more precisely, the theory of the mode of generation of practices (Bourdieu, 1977: 72).

So in his view, the missing link in many of our investigations has tended to be the concrete practices that both presume and create the categories that we think of as constituting the world. It is these that both put these categories into play and perpetuate them for future players. It is these that provide access to "the *dialectical* relations between the objective structures to which the objectivist mode of knowledge gives access and the structured dispositions within which those structures are actualized and which tend to reproduce them" (Bourdieu, 1977: 3; see also Benson, 1977). Nevertheless, because these "objective structures" are only sustained through the practices that *enact* them, there is no guarantee

that they are with us forever (see also Foucault, 1972; Giddens, 1984; Ranson, Hinings & Greenwood, 1980; Weick, 1979).

The implication of these methodological considerations is that we would expect to find researchers within both paradigms gathering their data through synchronic and diachronic investigations. This is because their differences, to the degree that they arise, are more likely to surface in what they subsequently do with the data thus collected. Whereas those carrying out synchronic studies in the Duality tradition might be likely to invoke such methods as multiple regression which presumes an underlying normal distribution, their Relationality counterparts might be employing correspondence analysis which looks only at relations *within* the data thus collected (e.g., Bourdieu, 1984; Van de Geer, 1993). Similarly, where the events of subjectivist studies might be interpreted as revealing the private realities of the subjects involved, within the Relationality paradigm these events would be seen in terms of the ongoing negotiation of inter-subjective and thus public realities (e.g., Gephart, 1978; 1993). But in the end, what would most differentiate the researchers who have adopted the Relationality paradigm would be their insistence that neither of these approaches is of itself sufficient; that what is necessary is a third "moment" of analysis in which the findings of the other two are brought together (Phillips & Brown, 1993; but see also Barley, 1990; Benson, 1977; House, Rousseau & Thomas, 1993; Pentland, 1992; Ranson, Hinings & Greenwood, 1980; Rousseau & House, 1994).

Problematic: What are we trying to explain?

Thus far I have identified the respective metatheoretical assumptions of the Duality and Relationality paradigms. But as I noted above, we seldom speak of our research in metatheoretical terms. In fact, it might even be said that our only occasion for speaking in these terms is when we feel ourselves to be in some way at a theoretical impasse. And even then, having thus far been without a metatheoretical alternative, our usual strategy has been

"puzzle-solving" as opposed to "extraordinary science" (Kuhn, 1970); it has been to seek a solution from within our theoretical perspective as opposed to somehow "rising above" it to see if the problem does not lie elsewhere. So for this reason, it is rare for us to find research that makes its metatheoretical assumptions explicit. And as a result, it can be difficult to discern which of the two paradigms articulated herein that a given piece of research may be indicative of. For as was just described, even the methodologies of a given investigation do not provide us with an unequivocal answer to this question.

Nevertheless, in most cases the relevant paradigm can be quickly discerned through an examination of the question that the research in question seeks to answer. For to speak in terms of genealogical rather than ontological categories as the Relationality paradigm encourages us to do is to describe the objects through which we experience the world as though in a continual state of becoming (Benson, 1977; Nehamas, 1985; Ranson, Hinings & Greenwood, 1980). And to accept that objects are in a continual state of becoming when a great many of the objects through which we know the world appear to be immutable is also to accept that what must be accounted for through our research is the *stability* of the forms around us. This, however, is in stark contrast to the descriptions associated with Duality where the objective is to account for the means whereby *change* in these forms might come about. For taking the subjects and objects of reality to be the things of the world, the vocabulary of Duality encourages us to speak in terms of subjects acting upon objects, as though this were the only means by which change might take place. Thus, when we adopt the vocabulary of Duality we describe our research as providing theories of change, or, at the very least, as revealing the laws within which change unfolds. By contrast, when we adopt the vocabulary of Relationality we accept that change is inevitable and that what is required is, instead, a theory of stability (Shotter, 1993).

Thus, examples of research in the tradition of Relationality can be found throughout the literature of our discipline. For example, Barley's (1986; 1990) research provides us

with an insightful window into how ways of behaving that will eventually be taken-for-granted come into being. Similarly, Pentland's (1992) illuminating account of 'organizing moves' allows us to appreciate how the mysterious but nonetheless real entities we call "organizational structures" are sustained through our day-to-day activities. Alternatively, discursive studies such as those of Gephart (1978; 1993) help us to see that even our most cherished entity – our Self – might only persist as a result of our speaking of it in particular ways. And at a more macro level, the stream of research known as Institutional Theory (e.g., Meyer & Rowan, 1977) seeks to illustrate that the persistence of particular organizational forms is not always (if ever) attributable to naturally occurring forms or economic rationale.

And yet, if research is already being carried out in the manner suggested by Relationality, then why would we want to articulate this paradigm and speak in its terms? The answer, in brief, is that it might allow us to see that research that was previously understood to be incommensurable may in fact be integratable. For within the language of Duality, it is unlikely that the examples given above would have been put together. Instead, discursive investigations would most likely be portrayed as subjectivist, while most Institutional inquiries would be seen as objectivist. The result, of course, is that few would consider that they may have something to offer each other.

But it is exactly this that leading researchers of Institutional Theory are struggling to say (e.g., Powell & DiMaggio, 1991a). But trapped within the language of Duality they find themselves, like so many others (e.g., Barley, 1990; Pentland, 1992; Ranson, Hinings & Greenwood, 1980; Wilmott, 1993), unable to say what it is they wish to say. Instead, all they can do is tell us what it is not; e.g., that it is *neither* subjectivist *nor* objectivist. And it is for this reason that Richard Nice (1977: viii) has so aptly noted that, "a text which seeks to break out of a scheme of thought as deeply embedded as the opposition between subjectivism and objectivism is fated to be perceived through the categories which

it seeks to transcend." So hopefully the language of Relationality provides us with an alternative; a means whereby we can now say that our research is "relationalist" and not be forced to subsequently explain it *in terms of* subjectivism and/or objectivism.

SOCIAL SCIENCE SOLIDARITY

Solidarity is not discovered by reflection but created.

- Richard Rorty, *Contingency, Irony, and Solidarity*

A great deal of human history can be understood in terms of individuals' efforts to associate themselves with some groups while distancing themselves from others (Bourdieu, 1984; Simmel, 1957). In this regard, it is through the ways we talk about our research that we define our identities and, in effect, proclaim solidarity with some colleagues while distancing ourselves from others. In describing our research in the terms we do, we do not just help others to understand it. In addition, the particular words we use in doing so tell our colleagues whether or not there is something in our research for them. And furthermore, these words often determine who it is that we collaborate with and the kind of research we do. So talking *about* our research is considerably more than idle chatter (Sandelands & Drazin, 1989; cf. Sköldbberg, 1992). In an only somewhat indirect manner, it determines who we are, what we do, and who we are likely to become (e.g., Gephart, 1993; Townley, 1993; Weedon, 1987).

It is for this reason that, in the discussion above, I have tried to present us with an alternative way of speaking about our research and, hence, ourselves. I have tried to argue that if the language of Relationality were in use, at least a few among us would no longer declare themselves subjectivists or objectivists but, rather, "relationalists". And in doing so – in adopting the language of Relationality – we would no doubt find that the boundaries of our research domain have, to some degree, shifted. We would find that the sorts of

questions we ask and the manner in which we try to answer them have somehow changed. Thus, over time, having shifted our allegiances, altered both our questions and our methods, and having most likely reached different conclusions, we would also find that we had become someone very different than if we had continued to define ourselves as *either* subjectivist *or* objectivist.

But once again, this is *not* to say that describing ourselves in the language of Relationality is in anyway "better" than doing so in the language of Duality. Nevertheless, it *is* to say that describing ourselves in any terms will inescapably affect us. In addition, it is to say that we now have another alternative to consider that may or may not present a future picture of ourselves we find desirable. And it is also to say that, should we find this image of ourselves worth pursuing, its realization may be as easy as talking about our work and ourselves in different terms. For this reason, it is at least worth considering the image of ourselves we would like to bring into being.

It is at this point that the essence of what I am trying to say with this article becomes apparent. For in this article, what I have been trying to suggest more than anything else is that we do have choices to make, or, more precisely, that we have to make choices. As the 'pragmatists' (e.g., Dewey, 1938; Mead, 1962; Rorty, 1989) and 'phenomenologists' (e.g., Merleau-Ponty, 1962; Schutz, 1970) have been telling us for quite some time, what we call knowledge is not decided for us by nature but, rather, in light of what it is we hope to accomplish. Thus, as the gate-keepers of knowledge, we are, as academics, also the gate-keepers of what can be accomplished; we are, to some degree, responsible for what it is that transpires in organizational settings (Phillips, 1991). For this reason, our challenge in the future is to come to terms with and act upon this considerably greater responsibility.

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CONCLUSION

Science further presupposes that what is yielded by scientific work is important in the sense that it is 'worth being known.' In this, obviously, are contained all our problems.

- Max Weber, "Science as a vocation."

In the preceding chapters I have tried to convey some sense of how our discipline of organization studies might be affected were we to take seriously the ideas put forth in Wittgenstein's *Philosophical Investigations*. In each of the chapters I have tried to bring Wittgenstein's way of thinking, or at least my understanding of it, to bear on a topic of interest to organizational researchers. In this respect, each chapter was more or less intended to stand on its own; each was expected to make its own contribution to the ongoing dialogue that constitutes the field of organization studies. Despite standing side by side herein, each was written with the expectation that it would at some point have to stand independent of the other two chapters now accompanying it.

When examined independently the contribution of each of the papers is, at least to my eyes, readily apparent. In the first chapter, written with the help of Nelson Phillips, the most obvious contribution is to be found in the effort to resist having the concept of 'language games' reduced to being merely a metaphor (cf. Beyer, 1992). However, in our attempt to accomplish this we also drew attention to how our tendency to think of organization science and management practice as two distinct and fully unified domains of activity impedes any understanding of the connections between them. By contrast, when understood instead in terms of collections of 'language games' engaged in by various groups of individuals, the distinction between them blurs, allowing the connections between them to come more fully into view. What we find, thus, is that, rather than organization science and management practice being distinct and singular activities, they are, in practice, merely the names we assign to the multifarious collections of social practices engaged in by those groups of people we tend to identify as "researchers" and "managers" respectively. In this sense, it is not the specific activities that differentiate these

groups but, rather, the particular set of activities engaged in and the manner in which they are brought together.

We end that chapter by extrapolating our argument from communities of scholars to the communities of individuals that comprise various organizations. In brief, we suggest that what we call an 'organization' may, in fact, be usefully understood in terms of its being a flexible network of language games. And although we only scrape the surface of this possibility, we do draw attention to the links that such a conception might allow us to make with many of the new and emerging currents within our discipline. So in this sense, the chapter as a whole can be seen as an attempt to divert a discussion that, in our view, was headed toward a *cul de sac* and, by doing so, to move it in a direction of almost endless possibilities.

With so many possibilities, though, the question quickly becomes, where do we begin? Thus, Chapter Two reveals where I believe we should begin, which is, quite simply, with a reconceptualization of the agent of organizational behavior. The metaphor of the information processor has, undeniably, allowed us to seemingly understand and anticipate a great deal of organizational behavior. However, with the insights thus provided, we have been led to ask questions of ever-increasing difficulty and complexity and, as might be expected, we have reached a point where the usefulness of this metaphor must now be called into question. Stated somewhat differently, what I am suggesting is that, as with the ladder that allows us to reach our current position, the information processing metaphor must now be left behind so that we might search for a means to reach the next level of understanding.

In this regard, what I argue is that Wittgenstein's conception of language provides us with the necessary toehold to reach this next level of understanding. By moving us away from a correspondence conception of language, and toward the notion of language as a

series of sounds and markings we emit in order to make our way in the world, Wittgenstein helps us to see that we can never *know* the true cause of organizational behavior. Nevertheless, our understandings of how this behavior has come about are not without consequences and, in fact, it is our agreement in the language used to account for human behavior that, to a large degree, makes social interaction possible. Thus, what Wittgenstein's perspective allows us to see is that our understandings of the Self should not and cannot be determined by what actually is. On the contrary, they must be determined by what we hope will be.

But it is not just to the Self that this notion of describing things as-we-hope-they-might-be applies and, for this reason, in the third chapter contained herein I try to sketch out the contours of a new paradigm for organization studies, one in which we abandon the notion of essences and focus, instead, on the relations through which essences seemingly come into being. For those that choose to adopt this "relationality" paradigm, the starting point of any investigation will no longer be with *things* but, rather, with the totalities in which these things are able to emerge as such. In this view, if things appear to us as immutable, or even semi-permanent, this is not because they somehow "exist". Instead, the understanding is that this is the result of various sets of relations remaining stable, most importantly, the relations between the viewing subject and the object thus perceived. So although the forms through which we experience our world may, for all intents and purposes, remain constant, as described here there is no reason to assume that what is will always be the case, nor that it need be the case.

Individually, then, each of the chapters contained herein stands to make its own contribution to the study of organization. Each examines a particular topic of interest to organizational scholars and attempts to reveal how that topic might be differently approached were we to take account of the ideas put forth by Wittgenstein, particularly those expressed in *Philosophical Investigations*. But that said, what of this dissertation as a

whole? After all, despite the different topics addressed in each of the three chapters, have I not admitted that all are, at least to some degree, indebted to the ideas of Wittgenstein? And if this is the case, is there not something more that can be said with respect to Wittgenstein's place within organization studies as a whole? It is to these questions that I would now like to turn.

In this regard, I would like to begin by noting that, even within the span of my relatively brief intellectual career, a career that at this point extends back approximately six years, I can already discern an increasing interest in Wittgenstein's work within organization studies as well as within the social sciences and humanities more generally. Moreover, due to the peculiarities of his life and ideas, Wittgenstein has garnered a considerable degree of interest amongst those outside the academy. As Terry Eagleton (1993: 5) describes the situation,

Frege is a philosopher's philosopher, Bertrand Russell every shopkeeper's image of the sage, and Sartre the media's idea of an intellectual; but Wittgenstein is the philosopher of poets and composers, playwrights and novelists

By way of an example, Eagleton notes that portions of the *Tractatus* have even been set to music. And lest this be insufficient evidence, we might note that it is Wittgenstein and selected details from his life and writings that form the basis for Philip Kerr's (1992) recent detective novel, *A Philosophical Investigation*. But perhaps the strongest testament to Eagleton's observation is to be found in the fact that England's Channel 4 was able to convince none other than Derek Jarman to direct the "documentary" they were looking to make about the life of Wittgenstein. That Jarman, who is considered by many to be among the most brilliant film-makers of his generation, should agree to such a project provides ample testament to the allure of both Wittgenstein and the ideas with which he is associated.

The details of Ludwig Wittgenstein's life are, indeed, quite fascinating. Born into one of Vienna's wealthiest families, a family that was at the centre of Viennese cultural life,

Wittgenstein's biography begins intersecting with other prominent individuals almost from birth. Some illustrative examples: A well-known portrait of Wittgenstein's sister, Margarete, was painted by Gustav Klimt; Ravel's Concerto for Left Hand was written for his brother, Paul, whose promising future as a concert pianist was destroyed when he lost an arm in the war. Thus, just by virtue of his membership in the Wittgenstein family, we can see that Ludwig's biography would command a great deal of interest.

Nevertheless, the details of Ludwig Wittgenstein's own life are as fascinating as those of his family. And as if to ensure that his life would be evaluated independently of his family's, Wittgenstein refused to accept his portion of what would have been a substantial inheritance, choosing instead to live an austere lifestyle for most of his adult years. Thus, gestures such as this, combined with others such as his periods of isolation in Norway, his love for American detective magazines, his habit of sitting in the front row of movie theatres watching Westerns after giving lectures and, for that matter, the thinking-aloud format of the lectures themselves, have all contributed to the mystique that surrounds him today. Moreover, given that we are in the midst of an historical epoch in which the genitalia of one's sexual partners is understood to be highly significant, Wittgenstein's apparent attraction to those whose genitalia matched his own has made him all the more interesting within some segments of society. Thus, in our time, a life such as this is fascinating in its own right, and when it is the life of a philosopher who revolutionized his discipline not once, but twice, it is almost inevitable that he should become the object of public attention.

But if Ray Monk's (1990) biography of Wittgenstein is to be believed, Wittgenstein would have been deeply troubled by all this interest in these quirks of his personality. Moreover, I suspect he would have been equally troubled by all the attention being given to his writings *per se*. For as Monk notes, Wittgenstein never ceased encouraging those he cared about to abandon philosophy; he never tired of telling them they should remove

themselves from the university. And although this might appear somewhat hypocritical coming from someone who devoted almost his entire life to philosophy, when examined from the perspective of Wittgenstein's life as a whole – at least the life portrayed by Monk – it is really not that surprising. For when we put aside the idiosyncrasies and the elite company he kept, and look instead at the life that Wittgenstein was trying to lead, we can understand what might have been behind such suggestions. We can see that he had little interest in the intellectual quibbles by which academic domains are traditionally defined. Indeed, for what we find is that Wittgenstein, despite his reputation as a philosopher, read very little philosophy himself. As a result, the question that most seemed to interest Wittgenstein was simply that perennial one of, "How ought I to live my life?" And it was his striving to answer this question that was behind both his seemingly odd behavior and his extremely insightful writings.

It would, however, be a mistake to see Wittgenstein's writings as merely the effect of this moral quest for, at least to some degree, they must also be seen as the cause of it. This, at least, is the conclusion I would like to draw, particularly in light of Bauman's (1993) discussion of what he calls "postmodern ethics". As with other postmodernists, Bauman has embraced the postmodern condition: that world-without-foundations that can be seen as Wittgenstein's legacy. However, unlike his counterparts – to say nothing of the elder Karamazov – Bauman does not take this to mean that "all is permitted". Of those who draw this conclusion Bauman would be likely to say that they are tossing out the baby of morality with the bathwater of ethics. For in a world without foundations it is not morality but, rather, the unattainable dream of a universal ethical code that must be abandoned. The important point, in other words, is that we need not abandon both. In fact, the seemingly counter-intuitive argument which Bauman wishes to make is that it is only by abandoning ethics that we can begin to become moral beings.

Not surprisingly, Bauman's (1995: 1) argument hinges upon his definition of morality which, for him, has to do with "the challenge of the Other, which is the challenge of responsibility for the Other, a condition of *being-for*." This notion of 'being-for' is, as well, fundamental to his argument. Drawing heavily upon ideas associated with Emmanuel Lévinas (e.g., 1969), Bauman's position is that each of us has within us what he calls the 'moral impulse'. By this he means that, when confronted with another human being, what exists within us is a sense of absolute, unlimited responsibility for this Other, a responsibility that comes before any responsibility to ourselves. So it is for this reason that Bauman conceives of being-ness as, fundamentally, a state of being-for.

On this point, Bauman (1995) goes so far as to conclude that we are, *existentially*, moral beings. In making this point, what he hopes to put across is that the human condition is, first and foremost, one in which we must confront our innate sense of responsibility for the Other. But that said, being a moral being does *not* mean that we are predisposed to choosing the 'good' of having accepted our responsibility for the Other over the 'evil' of having turned away from this responsibility. Rather, it simply means that our plight is that of *having to choose* whether or not to accept that responsibility. For as Bauman sees it, at every turn we are being confronted with the unspoken demands of the Other.

The challenges this poses for the moral being who wishes to act morally are twofold. Perhaps most obviously, there are limits to how much we can give to the Other, particularly when we consider that there is no shortage of Others with which to be confronted. And even if we were to give everything we had to these Others, as described by Bauman (1993), this would in no way alleviate us of our responsibility and our desire to do more. The moral condition is, unfortunately, insatiable; the inescapable fate of the moral being would appear to be the inexorable feeling that one could always do more.

And it is for this reason that we find, only somewhat paradoxically, that the moral being often wants nothing more than to escape from this condition of being moral.

Even if we resign ourselves to this never-ending state of having not given enough, the moral condition is likely to remain unendurable. Because acting morally involves a great deal more than simply choosing to accept one's responsibility for the Other. Once one has accepted this responsibility, one must then decide how to act upon it. For what we must not forget is that the absolute and limitless demands of the Other are *unspoken*. Even if they were articulated, our obligations would still not be met by merely providing whatever it is that was demanded. For in choosing to act morally, we are agreeing to accept complete responsibility for the Other and this, in many cases, requires that we *not* give the Other what they may feel they most need, whether it be a drink for the alcoholic, or a toy for the spoiled child. And as if making such decision were not tortuous enough, the plight of the moral being is further exacerbated by our ever-more-readily acknowledged inability to assess the consequences of our actions. Withholding a drink from the alcoholic may have seemed like the responsible thing to do, but once we discover that they opted for suicide rather than sobriety our confidence is bound to be eroded.

Confronted with the inability to do enough, to say nothing of our inability to know whether the little we can do is helping or hindering, it is inevitable that we should seek a way out of this predicament. And this, says Bauman (1993), is the impetus behind the pursuit of a universal ethical code. Were it ever found, an ethical code such as this would alleviate us of these feelings of never having done enough and never knowing whether we have done the right thing. By living within its dictates we would finally be able to get on with our *own* lives, comfortable in the knowledge that we were, at the same time, helping others get on with theirs.

Such was the guiding vision of modernity or, as Bauman (1995) chooses to identify it, *the age of ethics*. Having razed the foundations previously provided by religion, modernity was to erect a new code of conduct that would have a much more secure foundation in the form of Reason. Not only was it to be a more secure foundation, anchored as it was in science the understanding was that this new foundation would in fact be the *true* foundation for human conduct. And for a population that took seriously its sense of responsibility, an ethical code such as this seemed a worthwhile and noble goal to be pursued, regardless of what the pursuit might entail.

Or so it might seem. For as Bauman (1993) convincingly argues, the quest for an ethical code is as much an attempt to escape from responsibility as it is an attempt to confront that responsibility. Simply stated, what it amounts to is an attempt to replace the gnawing uncertainty that comes with responsibility for the Other with the peace of mind that comes from knowing (with certainty) that one has conformed to an agreed upon set of rules. So in this sense, we might say that the ethical project of modernity had little to do with facilitating morality and a great deal more to do with supplanting it. If the ethical project did support morality, it is, as Bauman (1995: 71) so aptly notes, in much the same manner that the rope supports the hanged man. This does not necessarily mean that the ethical project is destined to produce less moral outcomes for, as was already stated, the fact that we are moral beings in no way guarantees that we will engage in morally better action. Instead, it simply means that our actions are inescapably moral in nature. Thus, it is certainly plausible that, if the universal ethical code were found, adhering to its tenets as opposed to confronting our own morality could, in fact, yield a more desirable outcome.

The problem, of course, is that the universal code of conduct has yet to be found. Despite the efforts of our best and brightest, two hundred years of searching has produced little in the way of universal rules and, at least for the time being, we are continuing to function with the hand-me-downs of much earlier generations (MacIntyre, 1981). And

although this was for a time attributed to the difficulty of the task before us, more recently it is being taken as an indication that the project was destined to fail from the outset. Premised as it was upon the belief that science had given us the ability to glimpse the true foundations upon which we stood, interest in the universal ethical code was bound to wane just as soon as belief in foundations began to waver. And unfortunately for the ethical project, this was precisely the effect that the later writings of Wittgenstein were to have.

But as Bauman (1993) makes clear, the demise of the ethical in no way frees us of responsibility. Just the opposite, in fact. For where we could, for a time, escape the tell tale heart of moral responsibility by adhering to the anachronistic but nonetheless agreed upon remnants of outdated ethical codes, once we write off the possibility of an ethical code altogether, we have no choice but to tear up the floor boards and confront our moral responsibility face to face. Once again, this is not to say that we will, necessarily, attempt to be more moral in our actions, for this is not the nature of the moral impulse. Nevertheless, having confronted our responsibility – having wrested it back from those who were supposed to have alleviated it – we are at least in a position where we can once again see that we are, as Bauman suggests, existentially moral beings. And by being aware of this condition, we at least have the possibility of choosing to act morally– of taking responsibility for our responsibility. Thus, it is because this possibility is upon us once more that Bauman (1995) wishes to proclaim postmodernity *the age of morality*.

What Bauman allows us to see is that ethics and morality are not one and the same; they are not, as so many are wont to argue, two sides of the same coin. And because they are not, the choice between modernity and postmodernity, at least to the degree that we have a choice, is *not* a choice between a world with ethics and morality and a world without. If the choice must be expressed in such terms, then the choice between modernity and postmodernity really amounts to choosing between *ethics without morality*

and *morality without ethics*. And given that ethics, at least as described here, is/was intended to be a means to morality, it would appear that it is postmodernity rather than modernity that should be embraced provided we do, in fact, want to provide for the *possibility* of moral action. As I have repeatedly said, this still will not guarantee that the outcomes will be any more moral. Moreover, neither will it make moral decisions any easier. But what it will do, if nothing else, is at least return them to a state where they once again become *decisions*.

So in contrast with those who equate postmodernity with the 'demise of the ethical', 'aesthetic individualism', or simply 'anything goes', I would suggest that what postmodernity really amounts to is an intellectual maturity. Whereas modernity was about "conflict-resolution, and about admitting of no contradictions except conflicts amenable to, and awaiting resolution" (Bauman, 1993: 8), postmodernity is about acknowledging that conflicts and contradictions are endemic to social life. Where the modern mind sees the warts and blemishes in our societal complexion as that which must be removed, the postmodern mind seeks nothing more than a means of coping with them. Deferring once again to Bauman,

What the postmodern mind is aware of is that there are problems in human and social life with no good solutions, twisted trajectories that cannot be straightened up, ambivalences that are more than linguistic blunders yelling to be corrected, doubts which cannot be legislated out of existence, moral agonies which no reason-dictated recipes can soothe, let alone cure. The postmodern mind does not expect any more to find the all-embracing, total and ultimate formula of life without ambiguity, risk, danger and error, and is deeply suspicious of any voice that promises otherwise. The postmodern mind is aware that each local, specialized and focused treatment, effective or not when measured by its ostensive target, spoils as much as, if not more than it repairs. The postmodern mind is reconciled to the idea that the messiness of the human predicament is here to stay. (Bauman, 1993: 245).

And in this sense, Bauman is quite justified in describing postmodernity as simply *modernity without illusions*. For at least with respect to morality, the modern and the postmodern mind share the same ultimate objective. However, where the modern mind pursues this objective with the illusion that the goal might someday be reached, the

postmodern mind does so despite the impossibility, or at least improbability, of it ever happening.

What this would seem to imply is that the fate of the postmodern mind is to ask, seemingly in perpetuity, "How ought I to live my life?" And, indeed, this question is the conclusion with which the postmodern mind is most often identified (e.g., McCarthy, 1991). Whether it be described as "the aesthetics of self-invention" or "the construction of one's life as a work of art", what is really being said is that, in the end, each of us must decide for ourselves how it is that we are to live our lives. Unfortunately, what has made this conclusion so unpalatable to so many is that, too often, both by those that endorse it as well as those that oppose it, it is equated with surrendering ourselves to each and every passing desire, to indulging our every passion. It is seen as choosing the Brownian motion of human impulse over the lifetime trajectory of a 'moral quest' (MacIntyre, 1981).

But to draw such a conclusion is to leave the question unasked. It is to abandon the ethical codes of yesteryear without then going on to ask, "In relation to what, if not an ethical code, am I to live my life?" For as Taylor (1985: 34) notes, "We are selves only in that certain issues matter to us." Or as Smith (1996: 107) interprets this, "a person is a being for whom things *matter*." Either way, if things *matter* to us, they can only matter in relation to something else. And in this regard, I am joining Bauman and Lévinas in arguing that what matters to us as human and thus as moral beings is to choose our course of action *in relation to* the Other, or to take responsibility for the Other. Thus, it is because adhering to the ethical code of conduct was seen as our best means of achieving this that what came to matter during the age of ethics was that we follow the rules. Unfortunately, as the rules and laws proliferated, slowly demanding more and more of our attention, the unsurprising result was that we more or less forgot what *really* mattered to us. We forgot that what really mattered was the Other.

What we find, thus, is that declaring that it is up to individuals to decide for themselves how it is they are to live their lives is not to free them from the yoke of responsibility. Rather, it is to place this yoke squarely upon their shoulders. And nowhere do we see this more clearly than in the portrait of Wittgenstein that Monk (1991) provides for us. The picture he presents is of an individual who seemingly sought nothing more than to be a "decent" person. And in keeping with the discussion above, what we find is an individual for whom being "decent" became an incredibly onerous task upon realizing that there were no foundations to support him. Nevertheless, he never ceased trying. And for this reason I would say that, in Wittgenstein we find not only our template for postmodern philosophy but, more importantly, our template for a postmodern life. And this, in turn, may yet prove to be his greatest contribution.

So in much the same manner that the first chapter of this dissertation was intended to salvage the contribution that Wittgenstein's concept of 'language games' stood to make to our discipline, I would have to say that, at least to some degree, the intention with this dissertation as a whole, and particularly with this concluding chapter, has been to salvage the contribution that Wittgenstein's life stands to make to our own lives. For within our discipline, as in most others, we are often too quick in setting aside the 'moral quest' that has inspired those whose ideas we so respect. The most glaring example here is, for me, is Adam Smith, whose identity as a moral philosopher seems to have been stripped away at precisely the moment he was welcomed in by the economists. But if Smith provides the most glaring example, within our discipline of organization studies, the most significant example of this is undoubtedly Weber. For as Clegg (1996: 868) recently noted, Weber may be "the most moral philosopher of management." And yet, how many of us would think of describing him this way? Thus, because I saw myself as more or less introducing Wittgenstein to the field of organization studies, I felt I had to do whatever I could to ensure that the moral aspects of his writings did not meet the same fate as did Weber's.

So in addition to the contributions associated with each of the individual chapters, what I hope I have accomplished with this dissertation is the following. To begin with, I hope I have convinced those who consider themselves "postmodernists" that the so-called 'demise of the ethical' is not synonymous with the 'demise of the moral'. I hope I have convinced them of just the opposite. As for their opponents who, simply by virtue of their not having embraced postmodernity have come to be known as "modernists", I hope I have convinced them that postmodernity, if it is to be resisted, should not be resisted simply because it precludes the possibility of a universal ethical code. For as I hope I have made clear, it may well be that this outcome allows for a more moral result. So to this group I would say that, if postmodernity is to be rejected, it must be because its reasoning has been examined and found wanting. And if this is to be its fate, I implore my colleagues to reveal its weaknesses to me as quickly as they are able to. For until such time as this, it is only in the terms described herein that I find I am able live my own life.

But in a world without foundations, how am I to live my life? As a social scientist who feels a profound responsibility for those around him, what can be done to better the world around me? "Just improve yourself," Wittgenstein would say, "that is the only thing you *can* do to better the world" (quoted in Monk, 1991: 213). And if he is correct, then perhaps this dissertation will in some small way have contributed to a better world. For as Michel Foucault (quoted in Miller, 1993: 33) once said, "[o]ne writes to become someone other than who one is," and this is point in which I am in complete agreement. That said, I would hope that by writing this dissertation I have become someone who is not only *other* than who I once was but, in addition, someone who is somehow *better* as well.

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